Responses to Reviewer 2

We would like to thank the reviewer 2 for their constructive and insightful comments. We addressed all of these comments in the revised manuscript to improve its overall quality in line with the reviewer's recommendations. Our responses are in black, and the comments of the reviewer are presented in blue font.

General comments

With interest I have read and reviewed this manuscript. It describes improvements in the description of the surface snow and ice of ice sheets in ORCHIDEE, part of the ISPL model.

In my view this manuscript is, after some revisions, very suitable for publication in the cryosphere.

First of all, thank you for your positive feedback on the manuscript.

The largest proposed changes are, firstly, in the model description (sections 2 and 3), as I got (initially) lost in it and essential details are missing, see below. Secondly, the discussion and figures in sections 5.2 and 5.3, need improvements, which is also discussed below in detail. At the other hand, the proposed methods make sense to me, and the choices made on what is discussed, and what not, are good, so in my opinion no significant changes are needed on that point.

Major:

A) The model and experiment setup descriptions are unclear at points and too brief. In part, this is due to the chosen order to give first a brief overview of things in section 2, and dive further into it all in section 3. The current set-up implicitly assumes that readers more-or-less do know how ORCHIDEE works, but that is not necessarily the case (at least not for me). Therefore, I propose to restructure these two sections, namely

First a description of the (forward) ORCHIDEE model, including the description of the densification (current section 3.3.1) and surface snow density (section 3.2 - if this is part of the forward model). It is logic to me to start with brief descriptions (in words) of the parameterizations that has not been adjusted compared published versions (like heat diffusion, water retention, snow aging) and continue (in next subsections) then which elements that have been adjusted or needs to be described in more detail.

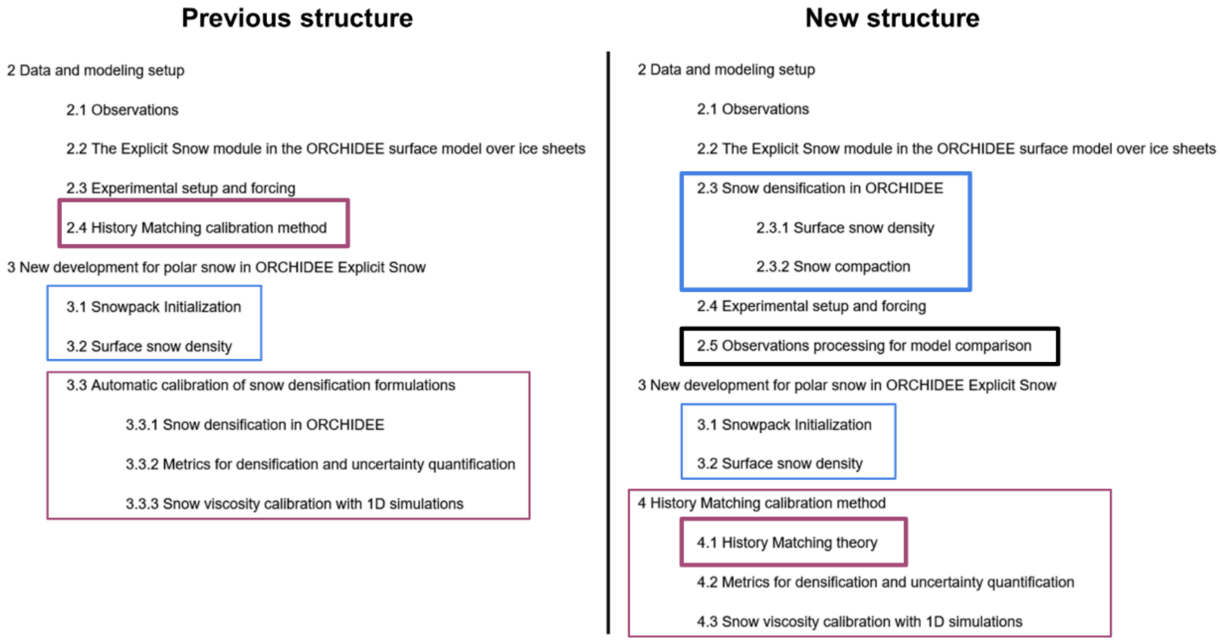
Second a description of the model/methodology to initialize ORCHIDEE (current section 3.1, and if the surface snow density estimation (current section 3.2) is not used in the forward model, also this.

Third a description of the calibration strategy, current sections 2.4, 3.3.2, and 3.3.3.

Fourth a description of the experimental setup and forcing, current section 2.3. Here, it remained unclear to me how the forcing with MAR data is carried out, remains

unclear to me. It is stated that ORCHIDEE calculates a SEB, but I don't see a description how this is carried out in offline mode, where the atmosphere model state and ORCHIDEE surface state does not align, especially when MAR hasn't been run with ORCHIDEE model. How are the surface energy fluxes are adjusted to close the SEB while following the atmospheric forcing? Please describe this in the revised manuscript.

Following your recommendations, we modified Sections 2 and 3 to create a clearer structure that is easier to read and understand. Here is a summary of changes in the structure:



Previous structure

2 Data and modeling setup
    2.1 Observations
    2.2 The Explicit Snow module in the ORCHIDEE surface model over ice sheets
    2.3 Experimental setup and forcing
    2.4 History Matching calibration method
3 New development for polar snow in ORCHIDEE Explicit Snow
    3.1 Snowpack Initialization
    3.2 Surface snow density
    3.3 Automatic calibration of snow densification formulations
        3.3.1 Snow densification in ORCHIDEE
        3.3.2 Metrics for densification and uncertainty quantification
        3.3.3 Snow viscosity calibration with 1D simulations

New structure

2 Data and modeling setup
    2.1 Observations
    2.2 The Explicit Snow module in the ORCHIDEE surface model over ice sheets
    2.3 Snow densification in ORCHIDEE
        2.3.1 Surface snow density
        2.3.2 Snow compaction
    2.4 Experimental setup and forcing
    2.5 Observations processing for model comparison
3 New development for polar snow in ORCHIDEE Explicit Snow
    3.1 Snowpack Initialization
    3.2 Surface snow density
4 History Matching calibration method
    4.1 History Matching theory
    4.2 Metrics for densification and uncertainty quantification
    4.3 Snow viscosity calibration with 1D simulations

As the reviewer suggested, we did the following changes in the structure:

- The new Section 2, “Data and Modelling Setup,” is now divided as follows: Section 2.1 “Observations” presents the observations used both for the initialization, calibration of the viscosity parameters and the validation of the new parameterizations; Section 2.2 “The Explicit Snow module in the ORCHIDEE surface model over ice sheets” provides a general overview of the snowpack model in ORCHIDEE, including a more detailed description of the model (in words).
- In Section 2.3, we now present the snowpack densification in ORCHIDEE. The section 2.3 is titled “Snow densification in ORCHIDEE”. This subsection is divided into two parts: 2.3.1 (titled “Surface snow density”), which presents the parameterization of surface densification in the model, and 2.3.2 (titled “Snow compaction”), which presents the parameterization of snow compaction in the model. These two subsections describe the parameterizations used in the previous version of the model, before the developments introduced in this study.

We believe it is necessary to introduce Section 2.4 (“Experimental setup”) in the following, as the grids and forcings need to be presented first in order to subsequently describe the post-processing of the observations, in particular the grid-averaging operations. So in this Section 2.4, we present the experimental setup, the description of the forcing fields obtained from MAR. It further describes the model configurations over Greenland and Antarctica with the different sets of albedo parameters. In addition, the section now presents the different ORCHIDEE experiments analysed in this study, namely OR OLD, OR OLD+INIT, and OR NEW.

Section 2.5 (titled “Observations processing for model comparison”) now corresponds to a new subsection concerning the processing of observations, particularly grid averaging. This clarifies the post-processing we performed and provides a coherent presentation following the discussion of the forcings, as requested by the editor.

Section 3 (“New developments for polar snow in ORCHIDEE Explicit Snow”) is now divided in two subsections: Subsection 3.1 (titled “Snowpack Initialization”), presents the new snowpack initialization method integrated into ORCHIDEE, and Subsection 3.2 (titled “Surface snow density”) is dedicated to the new surface density parameterization.

Next comes the calibration section 4 “History Matching calibration method,” which now contains now the three subsections 4.1 (“History Matching theory”), 4.2 (“Metrics for densification and uncertainty quantification”) and 4.3 (“Snow viscosity calibration in 1D simulations.”)

To address the reviewer’s comments, we have revised Section 2.2 (‘The Explicit Snow module in the ORCHIDEE surface model over ice sheets’) to provide a more precise description of the model:

“*Snow albedo is parameterized with the formulation proposed by (Chalita et al., 1994) depending on snow age, with a decaying exponential function from a fresh snow albedo value to a minimum albedo value (see Section 3, Eq. S3-5 in Supplementary). The snow age is computed through a dependency on snow temperature and is reset to zero depending on a fixed threshold of precipitation. Low temperatures slow the snow aging while temperatures near to the melting point increase the aging. The snow temperature is distributed across layers as a function of the snow thermal properties, with the snow thermal conductivity and heat capacity both parameterized as functions of density (Yen et al. 1981, Charbit et al. 2024, see Eq. S1-2 in Supplementary).*”

We also detailed the ORCHIDEE water retention principles : “*ORCHIDEE uses a bucket scheme regarding liquid water retention and percolation. The maximum liquid water holding capacity increases for densities above 200 kg m-3 (Boone et al. 2001), meaning that a denser snow can retain more liquid water.*”

We have also expanded Section 2.4 to better explain how the ORCHIDEE model is forced by the MAR outputs:

“The *ORCHIDEE model is forced every 3h in terms of solid and liquid precipitation, surface pressure, wind speed at 10 m, temperature and specific humidity at 2 m, downward shortwave and longwave radiation. At each time step, the model uses incoming radiative fluxes in the short and longwave bands from MAR, and computes the surface latent and sensible heat fluxes and the upward shortwave and longwave radiations. The model also computes the surface mass balance by accounting for solid and liquid precipitation from the forcings, as well as modeled sublimation and runoff over snow and ice.*”

B) ORCHIDEE is part of the ESM IPSL (state that more clearly in the introduction and model description, I hardly noticed it). Therefore, I understand the rationale to use climatological relations between the surface snow density and driving processes for the snowpack initialisation.

We have added a reference to the fact that ORCHIDEE is part of the Earth system model of the Institut Pierre-Simon Laplace (IPSL) in the model description :

“ *The ORCHIDEE model is the terrestrial component of the IPSL ESM.*” (Section 2.2)

However, I would say that during dynamic simulations for climates different than the "current one", one would like to use "non-present day" data as weather and climate during a simulation will/can be different than as foreseen during the initialisation. How could that be resolved? Please comment on this and this choice in the manuscript.

The first objective of the initialization procedure is to create a polar snowpack with realistic density characteristics at the first order, capturing the order of magnitude of the snowpack density in Antarctica and Greenland. However, we agree that this method for initializing temperature and density depends on the “present-day” observations used; this is a limitation of our method. We added a reference to this in the text in Section 3.1 (Snowpack Initialization) :

“*Attention must be paid, as this initialization was developed using observations from recent decades, making it potentially not appropriate for very different climatic contexts from the present-day one.*”

Minor:

29: "Increase in ice concentration". Please rephrase as I don't understand what you mean with it. Do you mean, more bare ice at the surface, or increase ice lens formation, or even something else?

Indeed, the formulation was misleading. This sentence refers to ice lens formation. It is corrected in the revised version of the manuscript : “*notably inducing an increase in ice lens formation*”

54: "years to centuries". As far as I know does densification take at least 10 years, even for extreme accumulation sites. Ice can form faster, but that is due to refreezing, and that is formally not densification.

We corrected this to “*decades to centuries*”.

L111: At this point in the manuscript, it is not clear why it is relevant that firn cores reach a depth of 10 m - for "full" densification models, one wish to have a profile ideally to a density of 830 kg/m3. So, mention why this 10 m is relevant for this study.

In this snowpack model, we have a limited number of layers (12 layers). If we wanted to fully represent a snowpack density profile, we would need to model a snowpack several tens of meters thick. This would result in the formation of very thick layers. To avoid this, we focus on the top few meters of the snowpack. Finally, the primary objective of ORCHIDEE is to calculate interactions between the surface and the atmosphere. This way, modeling the upper portion of the ice sheet surface appears sufficient to accurately represent key processes involved in calculating the surface mass balance (runoff, melting, refreezing).

L129: Please mention the typical depth of the snowpack for accumulation conditions. I guess it is 10-20 m, given the other numbers.

The snowpack thickness can vary from tens to one hundred meters depending on the accumulation conditions over Greenland and Antarctica (Amory et al., 2024; Van den Broeke et al. 2008). As an example, at South Dome, ice is reached at more than 100 meters below the surface.

1. The Firn Symposium Team, Firn on ice sheets, Nature Reviews Earth & Environments, 2024

2. van den Broeke, M. Depth and Density of the Antarctic Firn Layer. Arct. Antarct. Alp, 2008

We have added the following to the Introduction section:

*"In Greenland and Antarctica, the ice sheet snowpack can reach several tens of meters (Amory et al., 2024)**,** and can locally exceed 100 m, like at the South Pole where snow densification occurs at a slow rate (Van Den Broeke et al., 2008)."*

L139: The order of retention and percolation does not matter that much if the time step is not too long, namely that the meltwater production during a time step is less than the retention capacity of layers. I'm surprised that switching the order makes such a difference. However, most importantly, this is discussed in detail later in the study. So, refrain here from conclusions but simply state here the code change and that the impact is discussed later in the manuscript.

The difference does not result only from the order of retention and percolation. Previously, refreezing was computed before liquid water percolation, which meant that only the meltwater present within a given layer could refreeze. As a result, the maximum amount of water that could refreeze in each layer was limited by the maximal retention capacity of each layer, rather than by the actual energy available to refreeze it. We corrected this approach: liquid water now percolates first in a layer, and refreezing is calculated afterward, before the retention is done. This allows the snowpack to refreeze all available water according to its cold content, rather than being artificially restricted by the retention capacity of individual layers.

We deleted this sentence in the Section "Explicit Snow" : "*As a result, the snowpack can now refreeze a larger amount of liquid water as long as sufficient energy is available*".

L147: "Energy balance": That is commonly used for the surface energy balance, but it is uncommon for a subsurface layer. More common is, IMHO, "cold content".

We modified this sentence by changing "*energy balance*" by "*cold content*" (l.139)

L155-157: It looks like information got here twice, remove duplications.

Indeed, the sentence was duplicated. We removed this error and kept this sentence:

"*Since the MARv3.13 forcings were not available for Antarctica at the time this study was initiated, we use the MAR version 3.12 for Antarctica, spanning 1980-2019 with a 35 km horizontal resolution (Davrinche et al. 2024).*"

L220: Please note that the relations of Ligtenberg et al (2011) are updated in forthcoming studies and that their exact values depend on thinks like "surface climate biases".

Similarly, we have also updated the scaling factor values to obtain better results. We added the following sentence after the Ligtenberg et al (2011)'s equation (Equation 6 in the revised manuscript) :

"*This gamma term is generally calibrated to account for surface climate biases (Brils et al. 2022, Veldhuijsen et al. 2023).*"

L230: As the surface and 10m snow temperature are two different things, this 0.8 C difference is not a bias but simply a difference. It is a different situation if the modelled 10m temperature is compared with observed 10m temperatures, however, that is premature to present in Figure 2 (if the authors wish to present that too).

We corrected this error in the sentence :

"*When evaluated over both ice sheets, the parameterized surface mean temperature correlates strongly with the observed 10 m snow temperature, with a difference of only 0.8 °C and a correlation coefficient of 0.93.*"

L236: Please describe too what is done for locations where net ablation is expected. Is then also a 10 m snowpack described?

To determine the thickness of the snowpack in Greenland, we made a distinction between areas where net ablation is expected and the interior of the ice sheet. This parameterization is presented in detail in Appendix A. We added a reference to this snow depth initialization in the Section 3.1 (Snowpack Initialization) :

"*Our initialization method expresses the initial density profile as a function of latitude and surface elevation only, making it easily adaptable to any snowpack model. It also initializes the surface temperature of the snowpack as well as the snowpack depth, which is considered uniform for Antarctica and variable for Greenland between the interior of the ice sheet and areas where net ablation is expected (see Appendix A).*"

To summarize the initialization of snow depth, we used the available density observations over Greenland, which were fitted in order to estimate the depth at which a density of 600 kg/m³ is reached. This criterion is used to distinguish dry snowpack conditions, for which a density of 600 kg/m³ is generally reached well below 10 m depth. Consequently, when this density is reached above 10 m, we assume that the area is subject to significant ablation, with melting and refreezing processes contributing to the densification of the snowpack. Based on this criterion, we used the surface temperature parameterization described in Appendix A to derive a parameterization of snowpack thickness. This approach allows for a spatially variable snowpack thickness at initialization, with differences between the interior and peripheral regions of the ice sheet. For Antarctica, we assume a uniform snowpack thickness across the entire ice sheet, as surface melting is negligible over most regions.

L384: Please motivate why 40-year average surface snow density is used. I'm not sure if (each of) the observations can be linked to a certain date (it should be, though), and if that is possible it would be more proper to take the surface snow density as modelled for that location and specific date (both by approximation, if needed). The surface snow density exhibits a seasonal cycle, especially when melting occurs, and now this cycle is ignored. This exact-time-fixing might also remove the need to "partially exclude" melting locations. It will not change that the new version is much better than the old one.

Thank you for your insightful comment. It would indeed be preferable to link the observations to their exact dates in order to capture the weather conditions at the time of precipitation. However, we face certain limitations that have led us to use a 40-year climatological average. We use surface density observations that do not all include the exact dates when they were collected. Since our density dataset is fairly limited, we wanted to have a balance between representativeness and the amount of data. For these reasons, we chose to make a compromise and study the representativeness of our parameterization over a long timescale.

L405: You might to consider to test the IMAU-FDM fresh snow estimate for Greenland as well (Brils et al, https://doi.org/10.5194/gmd-15-7121-2022) to strengthen the point that the single formulation suitable for two ice sheets is hard to get.

Thank you very much for your recommendation. It is a very good idea to highlight the fact that it is difficult to obtain a formulation suitable for both Greenland and Antarctica. We have therefore implemented the surface density formulation used in Brils et al., 2022 and derived from Ligtenberg et al. 2018, which is based on instantaneous surface temperature. To do this, we adapted Table 4 and Figure 6 to include a panel with the formulation from Ligtenberg et al. (2018), developed for Greenland. As shown in Figure 6d (revised version), this formulation yields good results for Greenland but significantly underestimates values for Antarctica. The mean bias of the parameterization is thus -22.8 kg.m^{-3} and the RMSE captures high variability with a value of 69.3 kg.m^{-3}. When we decompose the signal, using metrics differentiated between AIS and GrIS, we see that for Greenland we obtain a RMSE equal to 47.4 kg m-3 with the smallest bias among all the tested parameterizations (11.9 kg m-3), while for Antarctica we have a higher RMSE associated with a strong underestimation of surface density (RMSE = 99.8 kg m-3 and bias equal to -92.7 kg.m-3). We can conclude that this formulation is well adapted to represent Greenland conditions but not for Antarctica.

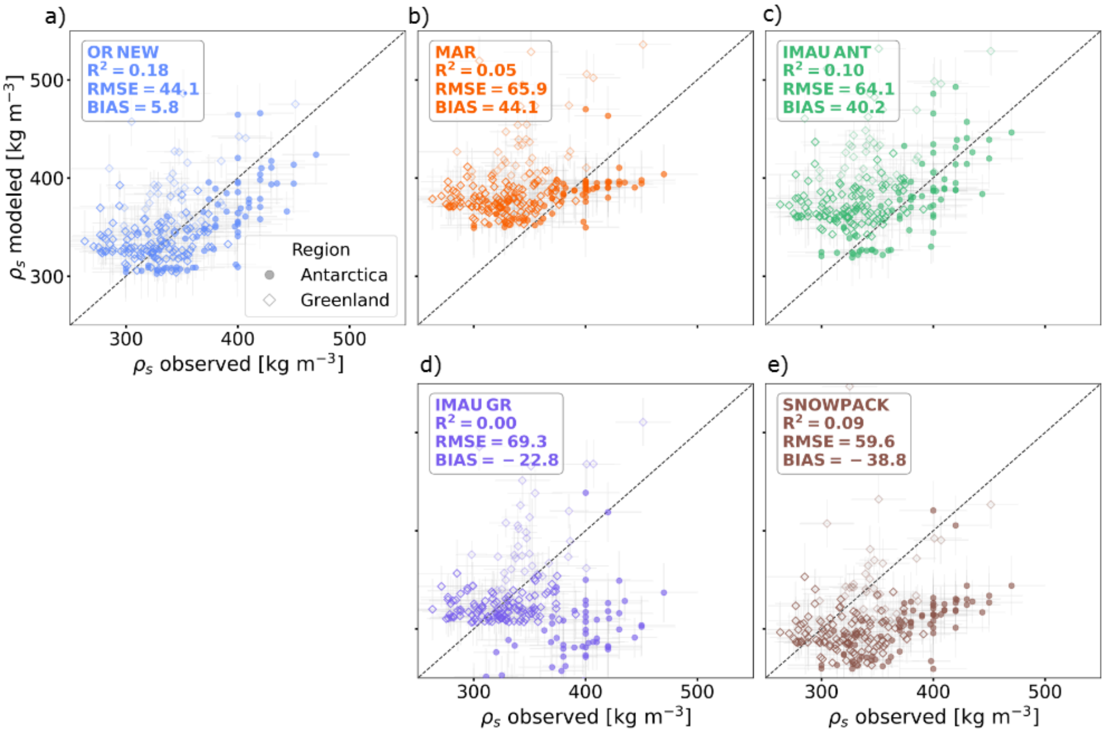


Figure 6. 40y-average surface snow density compared to observations, modeled by (a) the new wind parameterization in ORCHIDEE, (b) the wind parameterization developped for Antarctica in MAR (Kittel et al., 2021), (c) the wind and temperature parameterization developped for Antarctica in IMAU (Lenaerts et al., 2012; Veldhuijsen et al., 2023), (d) temperature parameterization developped for Greenland in IMAU (Ligtenberg et al., 2018) and (e) the wind paramerization developped in SNOWPACK (Groot Zwaaftink et al., 2013). Locations are specified with open diamond and solid circle, for Greenland and Antarctica respectively. Shaded points are locations where the 40y-averaged refreeze in the top 20 cm of the snowpack is larger than 0.1 mm d^{-1}. Error bars represent observational uncertainties (see Supplementary).

Figure 6 caption: "Superior to" -> "larger than"

We took into account this correction : “larger than”

L420: State directly that this -2.3 kg m-3 relates to figure 7a-left and 5 m snow density, and not in the second sentence.

This has been corrected in the revised version :

“*The agreement between ORCHIDEE outputs and observations is generally good,. At 5m, we note a negative bias of -2.3 kg m−3 (i.e.0.5% of the ρ5m average) and a RMSE of 42.8 kg m−3 (9.4 % with reference to the average ρ5m, Fig 7a left)*”

L437: Formally, the descriptions of the experiments (L439-441+Table 5) should be part of the experiment design, the fourth part of the new model & experiments section.

Thank you for your comment. As specified above, we have moved this description to the “Experimental Design” section (Section 2.4). We added a table to present the experiments and added the following text :

“*Following this, to investigate the effect of the developments proposed in this study, we define three different ORCHIDEEconfigurations (Table 3). The reference simulation,* *presented as OR OLD, uses the same configuration introduced in Charbit et al. (2024). This experiment shows an underestimation of density at the surface and an excessive compaction at depth (Fig.1c, f) and starts the simulation from a snow-free state over the ice sheet. The OR OLD+INIT configuration uses the new initialization procedure (Section 3.1), while OR NEW further includes updated model physics, with the new surface density parameterization (Section 3.2) and the calibrated viscosity parameters for polar regions (Section 4.3).*"

We have moved Table 5 (now Table 3 in the revised manuscript) to Section 2.4.

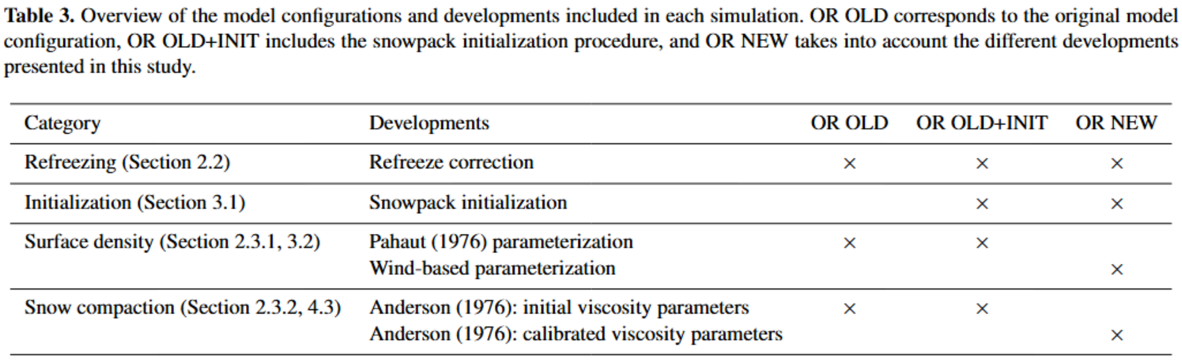


Table 3. Overview of the model configurations and developments included in each simulation. OR OLD corresponds to the original model configuration, OR OLD+INIT includes the snowpack initialization procedure, and OR NEW takes into account the different developments presented in this study.

Category	Developments	OR OLD	OR OLD+INIT	OR NEW
Refreezing (Section 2.2)	Refreeze correction	×	×	×
Initialization (Section 3.1)	Snowpack initialization		×	×
Surface density (Section 2.3.1, 3.2)	Pahaut (1976) parameterization	×	×	
	Wind-based parameterization			×
Snow compaction (Section 2.3.2, 4.3)	Anderson (1976): initial viscosity parameters	×	×	
	Anderson (1976): calibrated viscosity parameters			×

Figure 8, panels d-f: this is not an anomaly but difference.

We corrected the caption : "*Density difference*"

L465: this statement about thermal conductivity description should be not new, but already mentioned in the model description - and repeated (possibly shortened) here.

We added the reference about snow thermal conductivity and snow heat capacity in the model description, and we adapted the sentence :

« *In the ORCHIDEE snow scheme, snow thermal conductivity and snow heat capacity are expressed as functions of density (see Section 2.2 and Supplementary).*"

L480: Again, model setup details should not be introduced in the discussion of results. Please discuss OPT12L in sufficient detail in the model description section and focus in this section on the model results.

Thank you for this comment about model description. We removed this part from this section and added the presentation of the albedo parameters in the experimental design:

"*For these offline ORCHIDEE experiments over GrIS, we rely on 2 sets of albedo parameters. We first use the assimilated albedo parameters from MODIS satellite albedo observations products (Raoult et al., 2023), referred hereafter to as ASIM12L. However, Charbit et al. (2024) showed that the ASIM12L albedo parameters led to a significant underestimation of runoff over GrIS. Therefore, we also use the albedo- parameters set obtained from a manual tuning approach (Charbit et al., 2024) and designed to improve the agreement between the runoff simulated by MAR and ORCHIDEE. These albedo parameters will be referred to as OPT12L and are used only in Section 6.2*"

L485-514: The analysis of the authors is correct, but I find the provided figures not very helpful to showcase this. I would propose that they instead show in Figure 10 maps of modelled melt and refreezing for the old and new version (like panel 10a), as well as a map of modelled refreezing by MAR. Next, when the authors focus on 2012, they could better show profile timeseries of snow density, snow temperature and water content (and 1D timeseries of modelled melt and vertically integrated refreezing) for the period that refreezing is buffering melt water, to demonstrate why the new version can refreeze more water. See for example https://tc.copernicus.org/articles/14/3785/2020/tc-14-3785-2020.html, Figure 2, 4, 6 ... (but then not covering years but weeks). Add to the revised caption of which period panel 10a (and the new panels) are the average. If the authors do not adopt this change (which I would regret) please reconsider panels 10d and 10e, as the 3-hourly frequency leads to "brown area's" instead of clear difference between two simulations.

Thank you for your comment. We have taken your feedback into account and have redrawn and reorganized Figure 10. However, we believe that the melting and refreezing maps for the ASIM12L, OPT12L, and MAR simulations take up a significant amount of space in this section. We propose moving them to the Supplementary (Fig. S6 and S7) to avoid having a figure that is overloaded with information, and we will cite them in the main text.

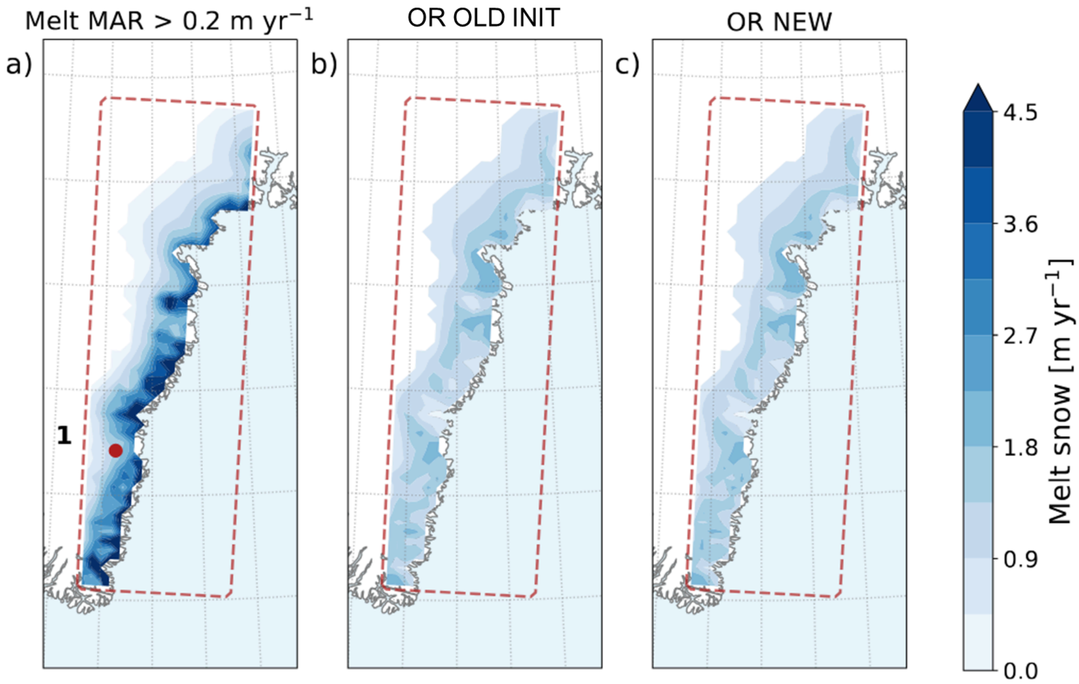


Figure S6. (a) Average melt from the MAR model over the southeast coast of the GrIS, where melt exceeds a threshold of 0.2 m yr^{-1}, between 2010 and 2019. (b, c) 10 years melt average from ORCHIDEE using the OPT12L albedo parameters over the same region for OR OLD INIT (b) and OR NEW (c).

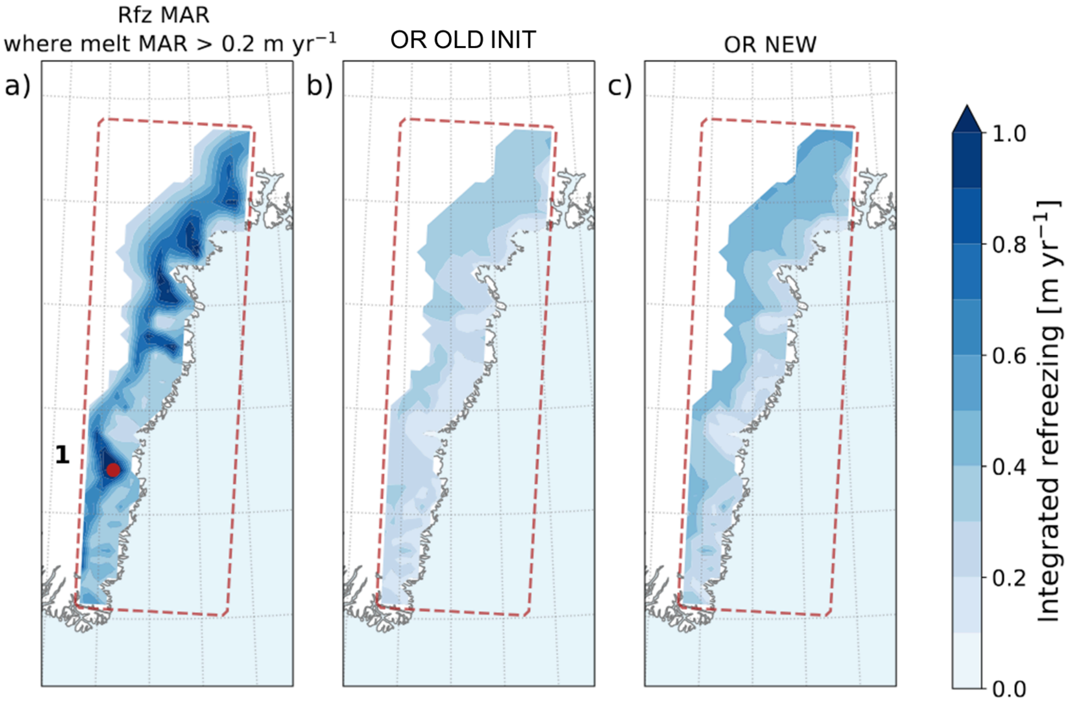


Figure S7. (a) Average refreeze from the MAR model over the southeast coast of the GrIS, where melt exceeds a threshold of 0.2 m yr^{-1}. (b, c) 10 years refreeze average from ORCHIDEE using the OPT12L albedo parameters over the same region for OR OLD INIT (b) and OR NEW (c).

We also replaced Figures 10 f, g, and h with a new panel inspired by the figure from Vandecrux et al, 2020. The main text has also been adapted to introduce this figure:

“*To better understand these differences, we analyze the 2012 melt season, which featured exceptional melt events across GrIS (Fig 10b). Although simulated melt rates are similar between OR OLD+INIT and OR NEW, refreezing differs notably early in the melt season (May–June 2012, see supplementary material). We further investigate the behavior of the model during the 2012 season at a representative site (Fig. 10a). We compare modeled snow density, temperature, liquid water, and refreezing concentration (computed respectively as the liquid and refreezing content divided by the layer thickness). As stated previously, OR NEW corrects the underestimation of surface density and the overestimation of compaction at depth (Fig. 10d),directly influencing the snowpack temperature and liquid water content (Fig. 10e-f). Because thermal conductivity is increasing with density, the revised density profile increases snow conductivity at the surface in OR NEW. This results in a higher cold content during winter and spring, with temperature differences ranging from -1.5 to -4.5 K between the surface and around 6 m depth (Fig. 10e). As refreezing is controlled by the available cold content, OR NEW promotes a more important refreezing of percolating meltwater (Fig. 10g). In particular, at the onset of the melt season, when the snowpack is still dry, the intense melt event of 3–14 May 2012 induces a pronounced refreezing peak, reaching 3 m yr−1 in OR NEW, nearly twice the rate in OR OLD+INIT (1.4 m yr−1), while the average melt amount during this melt event is similar in both simulations (4.4 m yr−1 w.e.in OR NEW in comparison with 4.8 m yr−1 w.e. in OR OLD+INIT). Throughout the melt season, refreezing rates near the surface remain consistently higher in OR NEW, with differences on the order of 8 to 10 kg m−3 d−1 (Fig. 10g). This increased refreezing in OR NEW directly* *lowers the liquid water content compared to OR OLD+INIT, as observed in May 2012 (Fig.10f) with differences of -6 to -9 kg m−3 between 4 and 10 m depth.*

In conclusion, the new developments promote higher refreezing rates by modifying the snow density and viscosity profiles. In OR OLD+INIT, the snowpack remains too warm to refreeze additional meltwater. In contrast, the colder snowpack in OR NEW allows for greater refreezing potential, which is consistent with the need to increase the melt and refreeze content in line with MAR."

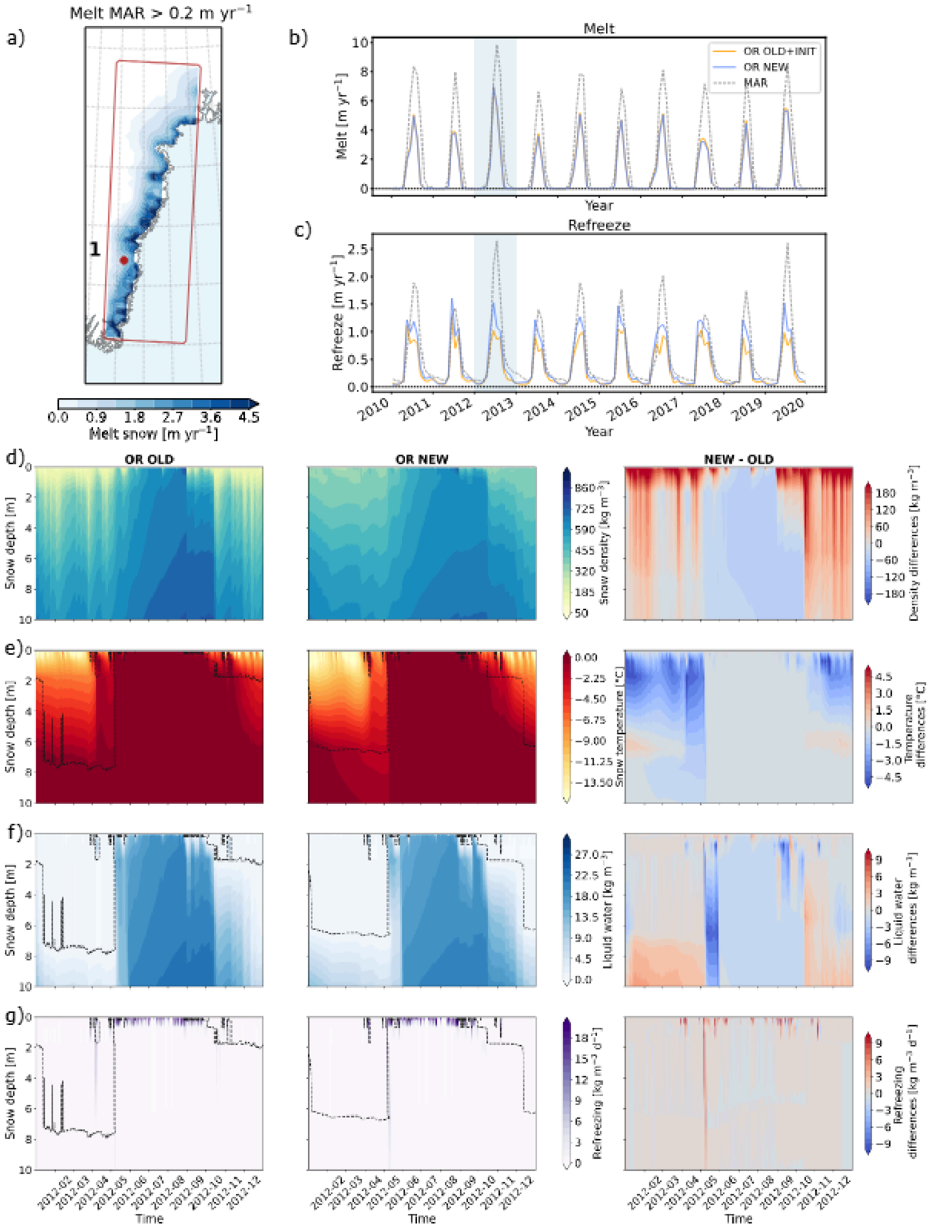


Figure 10. (a) Average melt from the MAR model over the southeast coast of the GrIS, where melt exceeds a threshold of 0.2 m yr^{-1}. (b–c) Mean seasonal melt and refreezing rates (m yr^{-1}), averaged over the region shown in (a). Results are presented for OR OLD+INIT (orange), OR NEW (blue), and MAR (dashed lines). The year 2012 is highlighted in light blue. (d–g)Simulated snow density (d), snow temperature (e), liquid water content (f), and refreezing (g), modelled with ORCHIDEE OLD (left), ORCHIDEE NEW (centre), and their differences (OR NEW - OR OLD, right). In panel (e, f, g), the black doted line represent the threshold where liquid water is present or not in the snowpack.

Section 5.3: The definition of ablation area is the area where (ice) mass loss exceeds mass gain, which is not equal that snow melt exceeds accumulation. Due to this IMHO incorrect definition of 'ablation area', the discussed points are in the lower percolation zone. Please adjust the naming of the region and not the discussed points (as snow densification in the true ablation zone is very dull - its only about seasonal snow).

Thank you for your insightful comment. Our definition of the ablation zone was misleading and incorrect. We are correcting this section by replacing “ablation area” with “lower percolation zone.”

The discussion of results in this paragraph is not very convincing. Yes, for a chance for modelling realistic firn profiles, correct surface conditions (melt+temperature+accumulation) are needed, but the authors do not show/discuss if ORCHIDEE got realistic values or not - please add this. Still, if correct surface conditions are provided, models can go easily err in the lower percolation zone, as the eventual density profile depends on the long-term history (melt is increasing over Greenland, so these firn columns are not in long term equilibrium) and the efficiency of water percolation, refreezing and runoff.

Thank you for your comment. We fully agree that modeling density profiles in the lower percolation zone is highly dependent on the history of melting, percolation, and refreezing.

Accumulation in MAR has been extensively studied and compared with observations, giving us confidence in the mass input provided to the model. Surface air temperature is also provided by MAR in the forcing. Regarding melting, we use the ASIM12L albedo set (see Section 2.4) , which underestimates melting (Charbit et al., 2024). Consequently, we are also underestimating the amount of refreezing, as shown Section 6.2 (“Evaluation of melt-refreeze processes”). The objective of this section here is to show that, for the lower percolation zone, other processes come into play in the densification of the snowpack, such as melt/refreezing cycles. Our goal is not to demonstrate that we represent densification in ablation zones in the best possible way. We want to highlight the fact that in this zone, we are particularly dependent on surface climate and thus on the chosen albedo model. Accurate representation of these zones goes along with the correct representation of the albedo which directly influences the melt/refreeze cycles .Previous efforts based on data assimilation of MODIS albedo products (Raoult et al., 2023) or on manual tuning (Charbit et al., 2024) have been made to simulate runoff in agreement with MAR. However, owing to our new developments reported in the present study, our results highlight the need for a new calibration of the albedo parameters. This will be conducted in near-future work.

If the authors really would like to show how well ORCHIDEE can model the firn layer in the percolation zone, the authors should mimic (in condensed form) the analysis discussion of the 'reference paper on this matter',https://tc.copernicus.org/articles/14/3785/2020/tc-14-3785-2020.html. Given that only one model needs to be discussed, a few panels would do. I like that the authos have one figure per section, retain that, but the proposed suggestions require some choices on what to show in Figure 11 - and what needs to be shown in the supplementary materials. Lastly, relate your findings on 'what is needed to model the

firn layer in the percolation zone right' to findings in existing literature, as the discussion is and will be too short for firm conclusions (which is fine).

We thank the reviewer for this insightful suggestion. Rather than adding this paragraph in the Section 6.3 (“Densification in the lower percolation zone”), we chose to address this point in the Discussion section, where it can be more appropriately framed and related to existing literature. We now explicitly discuss the role of meltwater percolation and the importance of preferential flow processes:

“However, this improvement still does not fully capture densification in ablation areas, where refreezing of percolated liquid water drives additional densification, especially in the first meters of the snowpack. As discussed in Section 6.2, both meltwater production and refreezing remain underestimated in ORCHIDEE. Therefore, accurately representing the percolation zone relies on correctly simulating these melting and refreezing cycles and, by extension, the meltwater infiltration within the snowpack.Different field studies suggest that meltwater infiltration can occur through different mechanisms, either forming a uniform advance of a wetting front or through localized preferential flow paths (Marsh and Woo, 1984; Pfeffer and Humphrey, 1996). However, the mechanisms of infiltration remain insufficiently observed. Complex models that include a representation of both types of flow tend to overestimate the depth of penetration, while bucket schemes appear to underestimate it (Vandecrux et al., 2020). As recommended in this study, field campaigns combined with laboratory experiments are needed to better constrain the conditions of meltwater infiltration.”

L536: "We firstly": start a new paragraph here, like you do in L541.

We corrected this in the revised manuscript.

Figure 11, S1, S2, and S4: please add dots and error bars to show the estimated 5 and 10 snow depth and the uncertainty as defined in Eq. 8-9. Formally, these uncertainties (thus error bars) should also be added in Figures 2, 3(?), 5a, 6, 7a, and 11a.

Error bars have been added to Figures 2, 7, 11, S1, S2, and S3 for the density profiles, as well as for the density metrics at 5 and 10 m depth. As multiple observations are not always available at each site, the uncertainty shown for the profiles reflects only the variability within each individual profile. For each metric, the standard deviation was calculated over observations within a 2 m interval around the metric, divided by the square root of the number of points. When fewer than 10 points were available, the interval was expanded to include the ten closest points to ensure a representative sample for computing the standard deviation.

For surface density, uncertainties were calculated for locations with at least five observations using the standard deviation divided by the square root of the number of observations. For points with fewer than five observations, the uncertainty was set to the average value computed from locations with five or more observations. In Antarctica, where uncertainties were sometimes provided with the surface density measurements, these were used to compute an average surface density uncertainty. The resulting average uncertainties are 28.3 kg/m3 for Antarctica and 24.3 kg/m3 for Greenland, which were applied to points with fewer than five observations in the upper 20 cm. We added both paragraph in the Supplementary to explain how the uncertainties were calculated.

The revised manuscript includes updated figures showing the uncertainty associated with surface density and with the density metrics at 5 and 10 m depth. Figure 7 illustrates the uncertainty associated with the observed 5 m and 10 m density metrics:

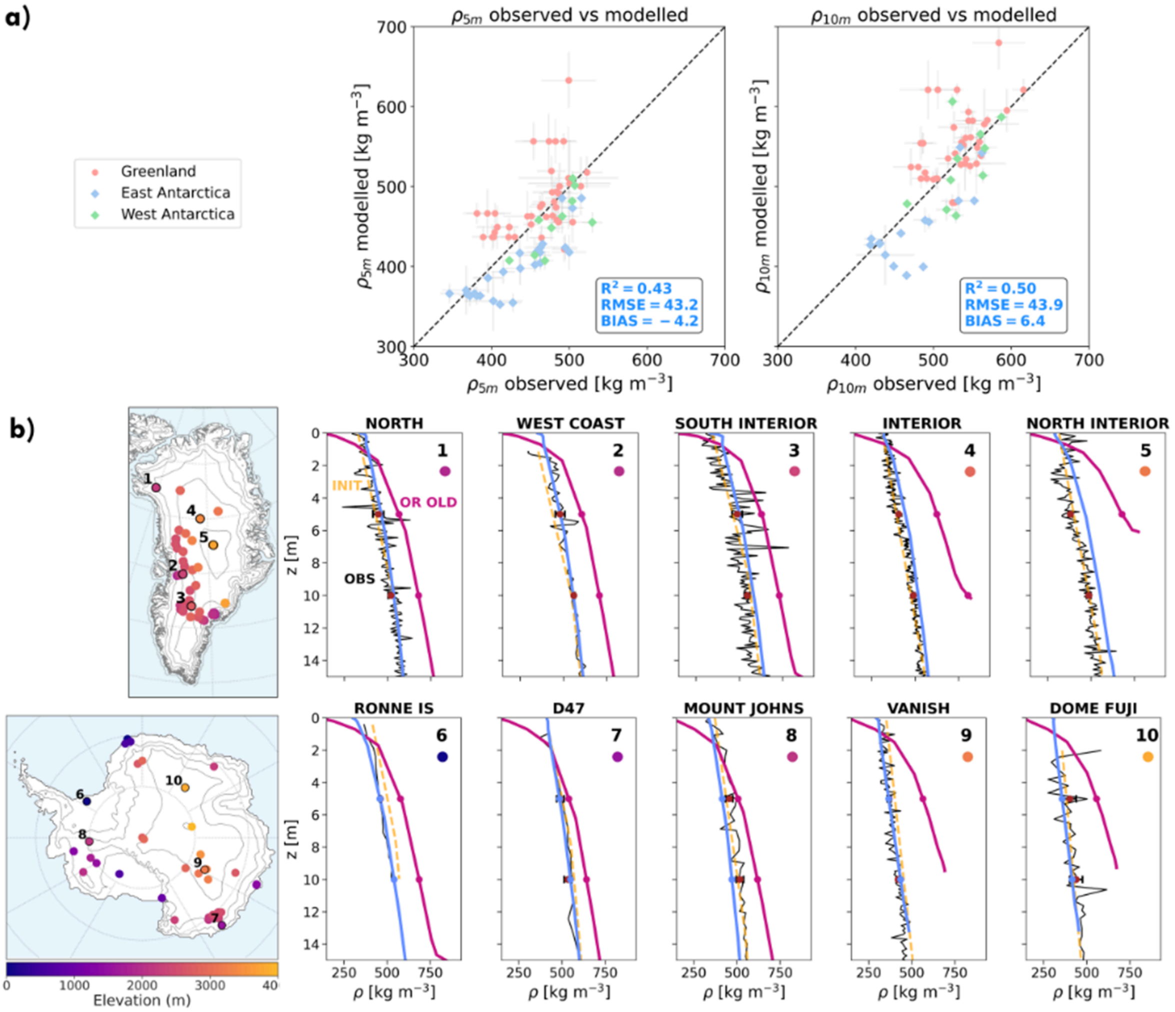


Figure 7. Comparison of (a) densities at 5 m and 10 m depth between observations and the ORCHIDEE model with the calibrated set of snow viscosity parameters across both GrIS and AIS. (b) Ten density profiles representative of dry snow locations across both ice sheets. Dark lines represent observed density profiles, orange dashed lines indicate the initialization profile, the purple curves (OR OLD) show the density profile before developments and calibration and the blue ones (OR NEW) represent the new density profile. Error bars represent observational uncertainties.

Figure 5 illustrates the uncertainty associated with surface density observations:

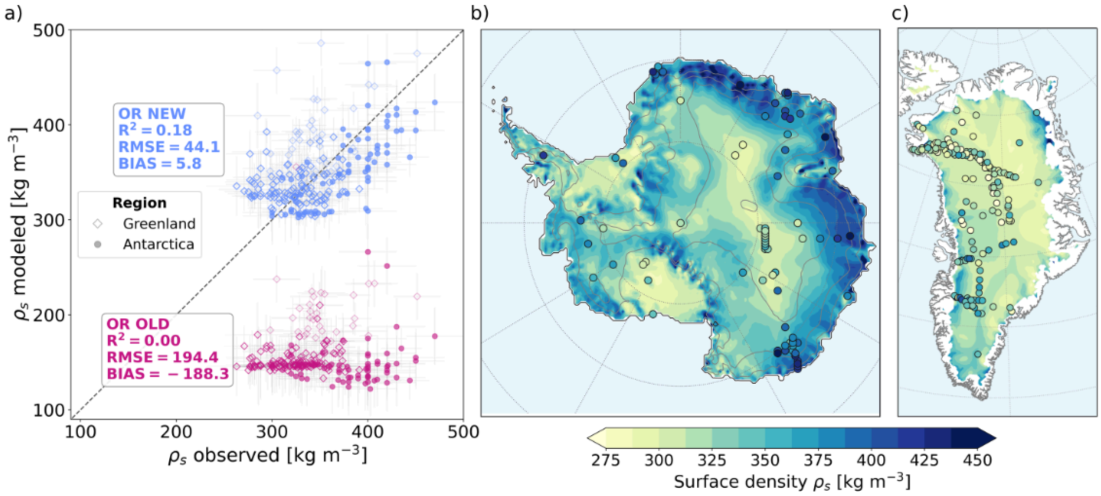


Figure 5. (a) Comparison of 40 year averaged (1980-2019) modeled surface snow density (computed as the average density of the top 20cm of the snowpack) with observed surface snow density from Greenland (open diamond) and Antarctica (solid circle). Model results are shown for the previous ORCHIDEE formulation (OR OLD,purple; Pahaut, 1976) and the new formulation (OR NEW, blue). Error bars represent observational uncertainties. (b, c) Spatial comparison of 40-year averaged modeled surface snow density with observations (in dots) in Antarctica (b) and Greenland (c).

L541: Make clearer that these changes are within the forward model of ORCHIDEE, not in the initialization procedure.

We modified the sentence to make clearer that this change is part of ORCHIDEE:

“*In addition, we introduced in the ORCHIDEE model a new empirical parameterization of surface snow density for polar regions, which is valid for both ice sheets and based on the 10 m wind speed.*”

L559-561: Such statements cannot be made without a proper introduction of the parameterization in the model description - so ensure the revised model description is detailed enough to allow statements like these. Now, something new is introduced in the 'conclusion', which should be avoided. Besides that, IMHO deeper refreezing in the lower percolation zones with ice lenses and ice slabs is more affected by the (in)ability of percolation water to reach all firn in a layer - ice lenses and ice slabs tend to make the horizontal water distribution very inhomogeneous.

We completely agree with this statement. We put this sentence in the description section of the model (Section 2.2) :

“*The maximum liquid water holding capacity increases for densities above 200 kg m−3 (Boone and Etchevers, 2001), meaning that a denser snow can retain more liquid water.*”

We keep this sentence in the discussion section : “ *Considering the influence of the liquid water retention capacity formulation (following the approach of Lafaysse et al. (2017)) may be an effective step towards a better representation of liquid water retention and refreezing processes. Indeed, alternative formulations (Lafaysse et al. 2017) simulate the opposite behavior as the formulation implemented in ORCHIDEE and have been shown to be consistent with laboratory experiments (Coléou et al., 1998).*”