Response reviews

We would like to thank the editor and reviewers for the comments we received. We combined the answer to the reviews into a single file. Based on the feedback, we made several changes that improved this manuscript. Some of the comments cover similar topics and we combined our answer to these comments. The reviewer's comments are displayed in black and our response is in blue. We refer to the track changes file for line numbers mentioned in this response. The line numbers of the reviewers refer to the preprint of this manuscript. In addition to the suggested changes, we made textual and notational improvements.

Specific IMAU-FDM and general firn model results

Table 1 does not add much in its current form. The list of papers and their time steps does not appear to be exhaustive, which makes the table potentially misleading. Simply referencing some examples in the text may be sufficient. Alternatively, the introduction could include a more informative table or figure that explicitly links model time-step choices to processes, firn model classes, or data availability constraints, etc.

Please consider rephrasing statements throughout the manuscript that pertain to the setup of the particular model tested here. For example, it would be helpful to specify which conclusions/results are for your model vs. those that can be generalized to the other types of firn models mentioned in the manuscript. For example, coupled models that calculate mass balance components themselves would not necessarily follow these results. This is inconsistent because many models are introduced in the paper (Introduction and Table 1), but then within the rest of the manuscript there is little distinction between which results could be extrapolated to which models/model type.

We give a combined answer to the two comments above. We removed the table, and instead referred to examples in the text. We also made the following textual changes:

- We stressed in line 47 that IMAU-FDM is forced with surface mass balance components at its upper boundary, to present the scope of this study.
- We clarified the distinction between findings related specifically to IMAU-FDM and findings that apply more broadly to firn modelling in Section 5 by explicitly stating the scope of each finding. Please see answers to the comments below for a detailed description of the changes we made.

Title: "on the modelled ice-sheet firn layer" sounds like the results presented here can be extrapolated to general modeling of the firn on ice sheets. While this might be true, it is unclear from the analysis, since so many results depend on the specific model being tested and its limitations. Please consider rephrasing. Maybe something like "on a modelled ice-sheet firn layer" or "on an ice-sheet firn layer model"?

We changed the title to "Impact of climate forcing time step in an ice sheet firn model".

Line 16: As an example of the points above: can you make all of these conclusions, based on the study done here, for any firn model? If not, then I think it is important to state here, for clarity, which results are for your type of firn model setup (i.e. one-way forcing of mass balance terms) and which are more general. Then, in maybe the discussion portion, it would be helpful to comment on which of those could be extended to a general firn model.

In general, we are more explicit about the results relevant for our model or any firn model. We removed lines 3 and 4 which might be misleading: "Firn models are forced with surface mass balance components, surface energy balance components and/or meteorological variables". In line 7 we added: "IMAU-FDM is forced at its upper boundary with surface mass balance components and meteorological variables". We change line 19 to: "firn models forced with mass balance terms and meteorological variables". In section 5, we rewrote the paragraph starting from line 325 to clarify that the parameterizations are specific to our model. Then, we present the more general conclusion that the original purpose and forcing time step of a parameterization should be respected and tested when applied. We changed the paragraph in the conclusion starting from line 356 to stress that the parameterizations are specific to IMAU-FDM.

Line 34: As mentioned above, I don't believe that you are testing the last two type of models listed. For instance, some models calculate mass balance terms, like melt, happening at different layers within the snow, and other models could simulate many more feedbacks not investigated here (i.e. processes like sublimation/evaporation simulated in-model). These models will likely have different requirements, especially for input temporal resolution. In light of this, please make sure these caveats are clearly articulated, to be clear to the reader the limitations of your results.

In line 47, we added that we use a model forced at the upper boundary with surface mass balance components and meteorological variables. In section 5, we discuss that our findings are not directly applicable to models that calculate the surface energy balance, but they might suffer form less accurate melt fluxes without a diurnal cycle in the climate forcing time step when there is coexistence of sub-zero temperatures and melt, that lead to immediate shallow refreezing.

Line 176 and Line 181: Please note here that this result is specific to your model setup (i.e. in a different model, melt might not even occur at lower temporal resolutions – for examples, if melt is calculated within the model).

Section 3.2 is specific to our model, whereas we discuss in section 5 how these findings could relate to other models. To make this distinction clearer, we renamed section 3.2 to "Processes responsible for Δ FAC in IMAU-FDM".

Line 260: This is well-stated, with reference the type of model fitting this conclusion. Please try to impart these type of statements throughout the manuscript and in the abstract.

We explicitly mention the model-specific and more general findings in the abstract, discussion section 5, and conclusion. We stress in the manuscript that section 3 and 4 are specific for our model.

Presentation of results in the conclusion and abstract

I think the conclusions would be more easily digested if they were more explicitly presented separately for the various climate/melt regimes outlined in the manuscript. I find that one of the more important results is that modeled firn behavior can be separated by regime, but I do not find that this is extensively articulated in sections like the abstract and conclusions. For example, there could be result statements that are for larger melt/MOA and then there could be those for lower melt/MOA.

In the original manuscript, we presented the conclusions structured by mechanism. We extended both the abstract and conclusions to discuss ΔFAC across different climate regimes by emphasizing the differences between melt and no-melt processes.

In the abstract in line 9, we added: "and that this effect depends on the climate regime." We also highlight what climate regimes are most sensitive: the regions with seasonal melt and emerging firn aquifers (line 10).

We restructured the conclusion. We added a paragraph that describes the effect of the climate forcing time step in different climate regimes (line 351). To make the conclusions easier to digest, the paragraphs either describe a mechanism or the effect of the climate forcing time step in different climate regimes. We are also more explicit about general findings and findings specifically for IMAU-FDM.

Lines 10-14 (and Conclusion section): I suggest that these conclusions be rephrased to specify that some of the results are particular to the model setup being tested in this study. As is, they are presented in a way that is a bit disorganized, with results from the different regimes mixed together. I think it would be more helpful to the reader to spit up the results in digestible categories. For instance, I think it is important to state that results depend on different regimes (i.e. runoff/climate), that some results could be generalized for firn models as a whole, that some results are specifically tied to spinup (i.e. they may be specific to chosen methods and not extendable to all firn models), and that some are tied to key temporal processes (refreeze) - hence they strongly affect the evolution of the forward historical runs. As the authors note in the text, this latter result would be the one most important for altimetry corrections. Please note that most of this information is within the text, but I think the abstract and conclusions, especially, could present them more effectively to the reader upfront.

Please see answers to the comments above and the answers below about the FAC results after spin-up changes over time.

Snowfall definition

The language surrounding "Snowfall" and Process 3 is confusing and needs revision. The term *snowfall* typically refers to a precipitation rate or mass flux, whereas this section is primarily concerned with the initial (fresh) snow density parameterization. Referring to this process as "Snowfall" is therefore confusing because the effect arises from how fresh snow density is prescribed as a function of meteorological variables that depend on time step. This distinction is important because elsewhere in the manuscript (e.g., in comparisons between Greenland and Antarctica), *snowfall* clearly refers to accumulation magnitude. Clarifying this terminology, or renaming this section, would substantially improve readability and reduce potential confusion. In fact, I am not confident that I have correctly deciphered the sections concerning snowfall.

We thank the reviewer for pointing out the inconsistent use of the term snowfall. We now clarify that accumulation represents the sum of snowfall, sublimation and drifting snow processes (deposition/erosion). The newly accumulated snow, if any, is assigned the fresh snow density. We implemented the following changes. In section 2.5 we added the definition of accumulation. In line 11 we replaced snowfall by snow accumulation. In line 148 we replaced snowfall by accumulation. In the revised Fig. 4, we explain that low 10 m wind speeds during snowfall increase fresh snow density. We renamed section 3.2.3 'Process 3: snowfall' to 'Process 3: fresh snow density on the APIS'. The remainder of section 3.2.3 consistently uses the terms snowfall and fresh snow density. In this section, we added the process described in the revised figure (Fig. 4 in the manuscript): with dt^{force} = 1d, higher wind speeds during snowfall lead to more crystal breaking, and therefore more efficient packing at the surface, increasing the fresh snow density. In the conclusion we removed that ΔFAC depends on snowfall and clarified that it depends on the fresh snow density parameterization.

Formation of ice layers

The model description needs to explicitly explain how ice layers are formed in IMAU-FDM. This information is essential for interpreting the results. For example, it is unclear whether daily forcing could artificially promote near-surface ice slab formation due to the way meltwater refreezing and layer formation are implemented.

Without a clear description of how refreezing transitions into discrete ice layers, whether those layers are permeable or impermeable, and how they evolve over time, the reader cannot fully evaluate the paper's conclusions regarding the impacts of model time step on firn structure and pore-space evolution.

In line 103 we will added a description of ice layer formation. In short: Ice layers have a density of 917 kg m^{-3} and form through refreezing of liquid water. The bucket scheme does not allow standing water over ice layers. Instead, water percolates downward to the next layer when no pore space is available.

Line 90. Does this statement assume that the entire firn column is at the melting point? How are ice layers handled in this context? This again highlights the need for a clearer description of ice-layer formation in the model.

Please, see our response above for the description of ice layer formulation and the proposed adjustments. During runoff events, all firn layers with densities below ice density contain liquid water and are at the melting point. The temperature of ice layers is determined through heat conduction.

FAC results after spin-up and changes over time

Please try to make it clearer to the reader that some results depend on spinup (choices/assumptions), and some of those spinup differences are model dependent. In addition, adding text and figures to specify whether those spinup differences actually affect the results of the forward historical runs would greatly help a reader better interpret the study results. For instance, should users of a firn products care about its temporal resolution (considering they care about FAC change over time)? What is the sensitivity of these results to temporal resolution, in what regimes does it matter, and for what types of models? These results are all touched upon in the manuscript, but I don't think a general audience would come away with clear guidance or answers to these questions. I suggest that they could more overtly be presented by including explicit quantification, extended figures, and more organized conclusions. Please see some specific suggestions below.

In the revised manuscript we clarified that the firn layer is initialized by spinning up the model over a repeated reference period until it reaches a steady state where the firn air content and elevation no longer change. The climate forcing time step in the spin-up and actual simulation are identical. The mechanisms described in section 3.2 also take place during the spin-up, which we added to the paragraph. Figure 9b shows that the ΔFAC is established during the spin-up, whereas Fig. 10d shows that the FAC changes over time differ between dt_{force} = 3h and dt_{force} = 1d. We added Fig. 11 to the manuscript (see Fig. 1 from this response) to show the influence of dt_{force} for two examples. Panel (a) shows that dt_{force} = 1d and dt_{force} = 1m result in up to respectively 0.2 and 0.4 m difference in surface elevation change, even though ΔFAC is primarily established during the spin-up. The example from the southern GrIS location in panel (b) shows a stronger divergence of surface elevation changes over time with values of 8 to 30% of the FAC. Altimetry corrections based on IMAU-FDM simulations with dt_{force} = 1d and dt_{force} = 1m give higher surface elevation increases for the two examples. Consequently, during ice thinning, subtracting the modeled surface elevation change from the altimetry signal leads to an underestimation of ice loss. During periods of ice thickening, it also leads to an underestimation of ice gain. Both examples demonstrate that users of altimetry data obtain different corrections based on dt_{force} when using IMAU-FDM output. To discuss these findings, we renamed section 4 to 'Implications for IMAU-FDM' and we add section 4.3 Altimetry corrections.

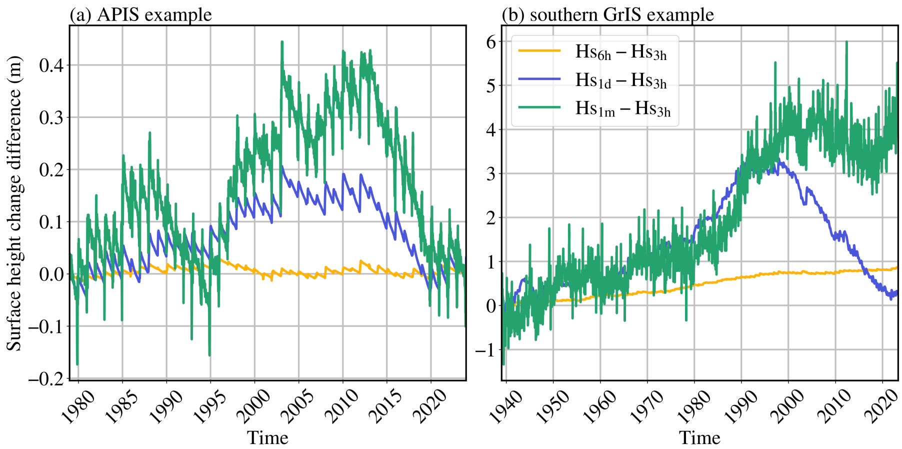

Figure 1: Maps showing the difference in elevation changes over time between the dt_{3h}^{force} and the simulations with larger climate forcing time steps.. Panel (a) represents the point in Fig. 9b and (b) the point in Fig. 10d.

Lines 142-143, Figure 3, Caption: Here I understand that you are showing the historical temporal average of the whole column. However, when showing mapped differences, that statistic has a bias in it, towards the earlier FAC states. Because FAC is cumulative, and a convolution of forcing and evolution over time. A more straight-forward comparison would be just to show the difference in FAC at the end of the simulation, since the last step FAC has all the cumulative evolution of the spinup and historical simulation already included in it. That being said, because the current maps illustrate mostly differences in the spinup states (as noted in the manuscript), I would suggest the authors add maps that show the FAC change during just the historical simulation. This is the diagnostic that a user of a firn product (eg, for altimetry) would find most important, i.e. something like dFAC/dt over the historical period. I realize that this might be a lot smaller, but I think it is important to present this change for users, and to explicitly quantify the difference in magnitude compared to the changes due to spinup only.

Although we agree that surface elevation change is most relevant for altimetry (corrections), the total FAC is also an important quantity for studies of e.g. retention potential; that is why in the revised manuscript we now address both aspects, by leaving Fig. 3 unchanged and adding Fig. 1 from this response. See our answer above for a detailed explanation on Fig. 1 from this response and the new subsection 4.3.

Line 155: Each of these processes impact the firn layer on different temporal scales. I suggest for each, that the authors clearly mention what the temporal scale is. This would help bridge the connection between processes that might be more significant on the scale of a spinup or processes that might occur over the historical period.

The processes described take place in every model timestep, also during the spin-up. We added in section 3.2 that these processes are computed every model time step, which is 15 minutes for IMAU-FDM.

Figure 9, Caption: Like mentioned for Figure 3 maps, I think it would be beneficial for the authors to add a plot of change in FAC over the historical (i.e. subtract out the beginning state). This would help illustrate if the FAC is diverging at all over time, or periodically, during the historic simulation. Spinup differences really hide this information, but I think its quantification is important to users of FAC, even if small compared to initial spinup spread. I realize that this is still not an apple to apple comparison between your different time step runs, because each simulation has a different starting state due to spinup, but I still think it is interesting to present, as long as that limitation is acknowledged.

Please see answers to the comments above

Line 256: Please explicitly state for the reader how this impacts altimetry estimates of mass balance.

Please see answers to the comments above

Organization

Section 3.1 (delta-FAC) is difficult to interpret when it is presented before the relevant physical context. At this stage in the manuscript, the reader does not yet understand the mechanisms responsible for the reported differences and so has little ability to absorb or evaluate the overall findings presented in this section. The manuscript would be more accessible if Section 3.2, describing the processes affected by time stepping, were presented first, followed by the resulting FAC differences. This structure would allow readers to interpret delta-FAC patterns in light of the governing processes.

Although we understand the reviewer on this point, we propose to stick to the order of sections. Instead, we explained the structure to guide the reader through the text more easily. Our motivation for the current section structure is that we first present the magnitude and pattern of ΔFAC to give the reader a feeling for FAC in both regions and the differences that a climate forcing time step cause. Section 3.1 provides relevance for further investigating the mechanisms behind ΔFAC. Moreover, we want to show the reader in what areas the dt_{force} has largest impacts. The idea behind this structure is to stress the motivation and provide context before diving into the mechanisms behind Δ FAC.

Conceptual overview Figure 4

While a conceptual overview figure is a valuable addition to the paper, Figure 4 requires substantial revision. In its current form, the schematic is difficult to interpret and appears inconsistent with the physical processes described in the text (to be blunt, it looks like something generated by AI). A few examples of shortcomings: In Process 1, the depiction of large refreezing grains at depth versus smaller refreezing grains near the surface does not appear physically meaningful and is inconsistent with the description in the text; In Process 2, the distinction between the left and right panels is unclear, and the intended contrast is not adequately explained in either the caption or the manuscript; In Process 3, it is not clear how the illustrated elements relate to time stepping at all. Given the importance of this figure for synthesizing the paper's key ideas, a clearer and more physically grounded conceptual diagram is needed.

We substantially revised this figure, see the revised Fig. 4 in the manuscript and Fig. 2 from this response. We made the following changes:

- Each process has a descriptive title that highlights the main finding, e.g. "1. Melt removes more FAC with smaller forcing time steps", instead of "Process 1: melt".

- In process 1, we illustrate melt with water droplets instead of the blue grains.
- In process 1, refrozen grains have a similar grain size and are shown in gray. The red shading indicates heat release.
- In process 2, we separate the two time step related processes that influence densification. One relates to the amount of firn, for which we increased the contrast between the dt_{force} = 3h and dt_{force} = 1d panel. The other process is related to heat release.
- In process 3, we relate the process to the time stepping.
- The figure is not made by AI and we made it more visually attractive.

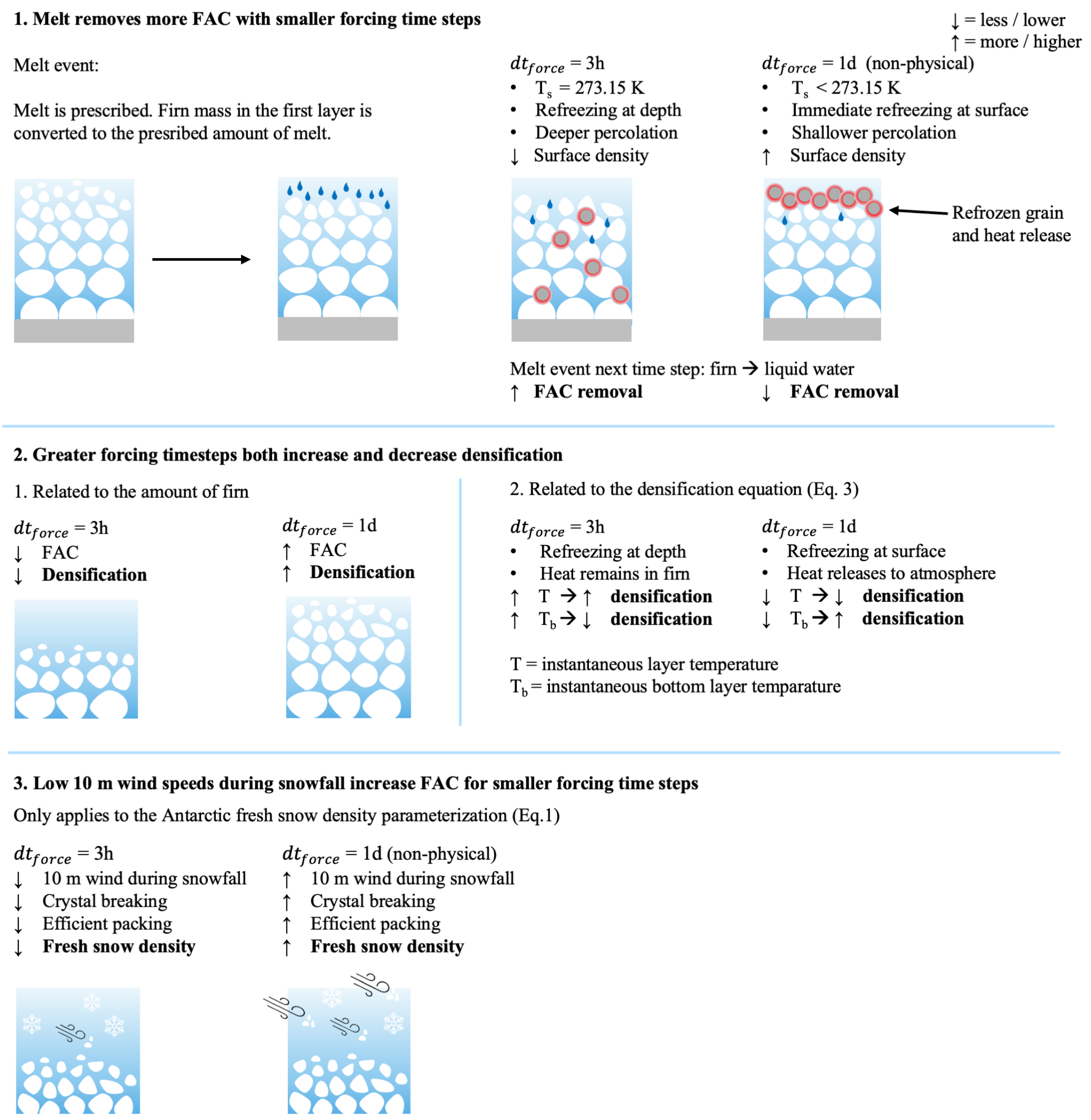

Figure 2: Processes that control (Δ) FAC.

Minor line by line comments

Line 17. The term *aliasing* would be appropriate when referring to distortion or loss of the diurnal signal.

We agree with the reviewer that the term *aliasing* describes the processes discussed. However, as the manuscript is already quite technical, we prefer to not introduce an additional term.

Somewhere in the introduction it may be useful to note that, while time step is often constrained by spatial resolution and numerical stability in atmospheric or ice-sheet models, this is generally not the case for purely one-dimensional firn models. Clarifying this distinction would help contextualize the choice of forcing time steps.

We do not fully understand the reviewer's comment. In our study we did not investigate numerical stability or sensitivity to the model time step. The model time step is 15 minutes in all simulations. The forcing time steps are 3-hours, 6-hours, 1-day, and 1-month and those forcings were linearly interpolated to the 15 minute model time step. As our analysis focuses on the effect of the forcing time step rather than the model timestep, we did not discuss the constrains for spatial resolution and numerical stability. We are also not confident that large model time steps can be used in firn models in general. In section 2.3, we stressed that we focus on forcing time step instead of model time step.

Line 133: Are these total FAC sum through the whole column (eg., down to close off, 830)? Please specify here for clarity.

In line 138 we introduce FAC for the first time. We added that FAC is the vertically integrated pore space expressed in meters in a column, representing the change in depth that would result from compressing the firn layer to the density equivalent of glacier ice (910 kg m^{-2}).

Figure 5, Caption: What is specifically meant here by "elevation change"? Please be more explicit about how this diagnostic is calculated, since it does not look to be the sum of the surface components. For example, is a mean mass balance subtracted out for ice flow divergence?

The 'elevation change' is the sum of the surface components, but we did not show the contribution of the ice flow divergence. We added this to the figure for consistency.

Figure 6, Caption: Please make it clear in this figure that melt is a forcing in your setup, and not modeled in this study, while refreeze is an output from this study's model.

We added to the caption and legend that snowmelt is input and refreezing is output.

Line 219: Please be more specific about the connection between firn and vulnerability here.

We replaced line 264 by "in assessing whether the firn air is depleted or could still buffer meltwater. Ice shelves are more vulnerable to hydrofracturing when the firn air is depleted. Therefore, neglecting the daily temperature and melt cycle in firn simulations could lead to underestimated ice shelf vulnerability."

Line 237: Does it have a higher heat content overall or a higher heat content at depth? Please specify.

The firn column has a higher heat content overall, as discussed in section 3.2.1. We clarified this finding by replacing line 283 "As meltwater refreezing occurs deeper for dt_{3h}^{force}, the firn layer has a higher heat content." by: "The firn has a higher heat content throughout the column for $dt_{force} = 3$h, due to the deeper refreezing of meltwater"

Line 200: "densification rate remains lower" might be more clear

We removed the sentence and added instead 'However, the reduction in firn depth resulting from densification (Fig. 5) remains smaller as less pore space is available to densify.'

We would like to thank the reviewers for the comments below. We incorporated them all. Please note that the line numbers mentioned here refer to the preprint instead of the track changes file. In blue we added the line numbers in the track changes files

Line 4. "In literature …" is awkward phrasing. Consider "In the literature" or "Prior studies …". (line 4)

Line 14: Along the lines of the previous comment, please specifically note here that the problems mentioned are for a wet system. (line 15)

Line 41. "We want to …" is awkward and redundant; the intent is already clear and "want" can be removed. (line 53)

Figures 3 and 10c: Can you add figure legends indicating the surface types as displayed at the top of Fig. 2? I think such a legend is helpful for readers.

Figure 5. The two dashed line styles are nearly indistinguishable; these should be revised.

Figure 7. Adding a 1 m depth marker to the top panels would help the reader interpret the figure.

Line 193: "firn" → "FAC" (?) as shown in Figure 3 (line 232)

Line 193: "less densification" → Please add a reference equation 3 here. (line 232)

Line 247: It would strengthen the dismiccuss here to point out that this result has strong implications also for estimates of mass balance and overall surface mass balance projections in the future. (line 296)