Response to reviewer 2

We would like to thank the reviewer for the comments we received. Based on the feedback, we suggest several changes that will improve this manuscript. Some of the comments cover similar topics. We will group them to give a combined answer. The reviewer's comments are displayed in black and our response is in blue.

Specific IMAU-FDM and general firn model results

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.

In line with the comments from reviewer 1, we propose to describe in the manuscript more clearly which conclusions are specific to IMAU-FDM and which to firn models in general. We propose to remove the table, and instead refer to examples in text. More specifically, we propose the following textual changes:

- We will stress in line 36 that IMAU-FDM is forced with surface mass balance components at its upper boundary, to present the scope of this study
- We will also extend the discussion section 5 by distinguishing between general conclusions and those specific to IMAU-FDM, to provide better context and reduce potential misinterpretation. For example, IMAU-FDM is forced at the surface, and we found that a climate forcing time step greater than a day allows for a non-physical coexistence of snowmelt and sub-zero surface temperatures, leading to immediate shallow refreezing of meltwater. This finding is relevant to other models that prescribe surface temperature and snowmelt, such as The Community Firn Model (Medley et al., 2022). Firn models with surface energy balance schemes that explicitly compute melt fluxes might be less sensitive to this finding, as long as the model does not allow coexistence of sub-zero temperatures and snowmelt under 1-day or larger forcing time steps.

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 will change the title to "Impact of climate forcing time step on modelling the ice sheet firn layer with IMAU-FDM".

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.

We will remove 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 will add: "The model is forced at its upper boundary with surface mass balance components and meteorological variables". We change line 16 to: "firn models forced with mass balance terms require a timestep small enough to capture at least the diurnal cycle". In section 5, line 264, we will clarify that the parameterizations are specific to our model. Then, we will present the more general conclusion that the original purpose and forcing time step of a parameterization should be respected and tested when applied. We change line 281 to stress that the parameterization findings are specific for IMAU-FDM: "For example, in IMAU-FDM, the fresh snow density parameterization determined the ΔFAC under limited snowmelt conditions and is related to dt^{force} for non-physical reasons."

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 29, we will add that we use a model forced at the upper boundary with surface mass balance components and meteorological variables. In section 5, we will add a new paragraph to discuss that the climate forcing time step in our model both influences FAC magnitude and its changes over time. We discuss that our findings are not directly applicable to models that calculate the surface energy balance. However, we will stress that modellers should be careful with 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 rename section 3.2 to "Processes responsible for ΔFAC 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 will clarify the model-specific and more general findings in the abstract, discussion section 5, and conclusion. We will stress 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 will extend 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, we will remove the following sentence (line 8): "and that locations with limited firn pore space due to seasonal melt are most sensitive." We will instead add: "and that the modelled firn layer response to the climate forcing time step depends on the climate regime. Locations with limited firn pore space due to seasonal melt, and regions with emerging firn aquifers, are most sensitive."

In the conclusion, we will remove the following sentence: 'The strongest sensitivities are found in climate zones with low FAC_{3h} or where firn aquifers are absent under larger forcing time steps, but present under shorter ones." We will instead add: "ΔFAC depends on the climate regime and is most sensitive to snowmelt. ΔFAC increases with snowmelt for melt rates up to 500 kg m^{-2} yr^{-1}, across both the APIS and southern GrIS. In the southern GrIS, snowmelt rates exceeding 500 kg m^{-2} yr^{-1} lead to the formation of firn aquifers. Lower climate forcing time steps result in earlier aquifer formation and therefore higher ΔFAC. Once aquifers have formed across the difference climate forcing time steps, ΔFAC increases with snowmelt up to a MOA of 1, above which the firn becomes depleted for all climate forcing time steps."

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.

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 clarify 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 mechanisms described in section 3.2 also take place during the spin-up. 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_{3h}^{force} and dt_{1d}^{force}. We will add Fig. 1 from this response (see below) to show the influence of dt^{force} for two examples. Panel (a) shows that dt_{1d}^{force} and dt_{1m}^{force} 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_{1d}^{force} and dt_{1m}^{force}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 will rename section 4 to 'Implications' and we add section 4.3 Surface elevation changes over time.

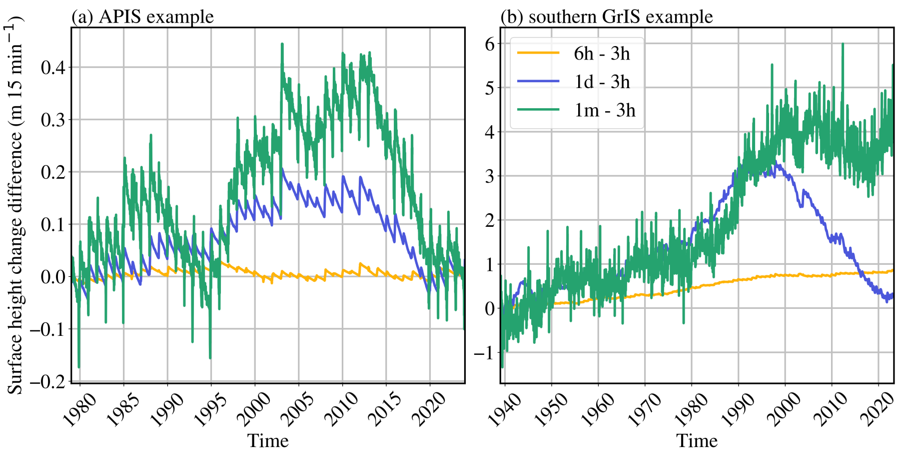

Figure 1: Maps showing the difference in elevation changes over time between the dt_{3h}^{force} and the simulations with larger climate forcing time steps.. Panel (a) represents the point in Fig. 9b and (b) the point in Fig. 10d.

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 will add 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

Specific comments and suggestions

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

In line 104 we introduce FAC for the first time. We will add 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 will add 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 will add 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 replace line 118-120 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 as 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 will clarify this finding by replacing "As meltwater refreezing occurs deeper for dt_{3h}^{force}, the firn layer has a higher heat content." by: "The firn column has a higher heat content throughout the column under dt_{3h}^{force}, due to the deeper refreezing of meltwater, which releases latent heat that is partly retained in the firn."

We would like to thank the reviewer for the following suggestions, we will implement them all:

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

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

Line 193: "less densification" → Please add a reference equation 3 here.

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

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.

References

Medley, B., Neumann, T. A., Zwally, H. J., Smith, B. E., & Stevens, C. M. (2022). Simulations of firn processes over the greenland and antarctic ice sheets: 1980–2021. *The Cryosphere*, *16*(10), 3971–4011.