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## Comment on tc-2022-118

Baptiste Vandecrux (Referee)

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Referee comment on "Characteristics of the 1979–2020 Antarctic firn layer simulated with IMAU-FDM v1.2A" by Sanne B. M. Veldhuijsen et al., The Cryosphere Discuss.,  
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Review of Characteristics of the contemporary Antarctic firn layer simulated with IMAU-FDM v1.2A (1979-2020) by Veldhuijsen et al.

B. Vandecrux, bav@geus.dk

The authors present a new offline run from their firn model, using both updated forcing (RACMO2.3p2) and updated firn model (IMAU-FDMv1.2A). The changes in the model are clearly described. The model output is thoroughly presented, including how it differs from previous version of the IMAU-FDM model. The output is compared to a multi-mission altimetry product over 1992-2015. The model is also run under various scenarios to test its sensitivity to uncertainty in the forcing data, model parameters, or in the choices made for the spin-up procedure. The manuscript is nicely written, and the figures are of very good quality.

However I have major concerns on the science output of the study and how it increases our understanding of the firn characteristics in Antarctica. The comparison to altimetry (although necessary and much appreciated) is non-conclusive because of the uncertainty of the altimetry product, and reveal that other, more precise, datasets should be used to evaluate the model. The sensitivity analysis shows that the firn model tuning procedure allows to fit equally well observations with different forcings. This makes it hard for other research teams to use the parametrizations developed here with any other forcing than RACMO2.3.p2. Testing the sensitivity on the spinup setting can be of broader interest for regional climate modelling community, but the study does not conclude on a best practice on this matter.

Nevertheless, the dataset it produces is of great interest for the science community, and actually has more value in itself than the science output being presented in the manuscript. This makes the manuscript perfectly suited for a data-oriented journal. For

the Cryosphere, I would encourage the authors to strengthen their findings. This could be done by exploring one of these three options:

- using new and hopefully more precise observation datasets to gain more insights on what the model is doing good or bad (coffee can experiments, GPS records, IceBridge laser measurements...)
- looking in more detail at regions where the model indicates changing processes
- detailing the sensitivity analysis so that the study can conclude in best practices that can benefit other research teams.

Maybe such scientific findings are already in the manuscript, and I have just missed them. Then I much apologize for my comment. More light should then be given to few key findings and some of the side analysis should be removed to keep the focus on the main insights. The findings should be highlighted in the abstract and in the conclusion. Once the interesting science findings properly highlighted, the manuscript will be a great candidate for publication in the Cryosphere.

Detailed comments:

- l.5 "observations" of what?
- l.8 "with altimetry" Please replace by "with previously published multi-mission altimetry product for the X-Y period"
- l.9 "reasonably well" Please quantify the agreement.
- l.19 Where does the 98% come from? Is it on average on the firn-covered area? At some locations, significant runoff can occur from the firn (e.g. ice slabs, perched or perennial firn aquifer regions)
- l.63 "measurements" of what?
- l.66 "remote sensing altimetry" please replace by "a multi-mission remote sensing surface height change product for the X-Y period (REF)"
- l.79 "further improved" do you use v1.2G as starting point? meaning do you use the same thermal conductivity as Brils et al. ?
- l. 83 "Kaspers et al. 82004)" How do they define the fresh snow density in their study? How long after deposition do they consider the snow to be fresh? Or if they look at surface snow, for which depth range? Was this study in Antarctica?
- l. 94 Same question as above for Lenaerts et al. (2012)
- l. 108 "average surface temperature" is it a fixed, long-term average or the average for the past x years? Is it surface skin temperature or near surface air temperature?
- l.115 "z\_830\*" Here you mention z\_830\* but in the next lines you only mention MO\_830. Is there a MO\_830\* ? I don't fully understand how z\_830\* is used. Please explain how MO\_550 and MO\_830 is used in Eq3.
- l. 120 What do you mean "optimize densification" ? To make the model match observed firn density? Please describe explicitly what is your objective function when fitting the alpha and beta parameter.
- l. 126 Please mention if there is any difference from v1.2G on this point.
- l. 127 "tipping-bucket" I think it is called simply "bucket". When the retention capacity of a layer (=the bucket) is full, the excess water flows to the next layer without the bucket/layer to "tip" or empty itself. It overflows, but it does not tip.
- l. 129-130 "if the latent heat..." Please rephrase to " and that has subfreezing temperature" or something alike.
- l. 134 "the reference period" Since you haven't specified it so far, maybe change to "a" reference period.
- l. 142 "a minor trend" Please give its magnitude to show that it is minor. Please explain briefly its origin. Is it because ice with air bubbles continue to replace the dense (917kg m-3) bottom ice prescribed at the initiation of the spinup? Later in the manuscript, you

mention that there is no trend in average surface height because you assume a steady state. Is the removal of the "minor trend" your way of prescribing steady state? I am wondering how this minor trend look compared to the trend you get when using your alternative spin-up strategy.

- l.147 Here I am a bit confused by the  $v_{ice}$  and  $v_{by}$  (for ice shelves only?) terms and by the frame of reference used.

Is  $v_{ice}$  the vertical velocity of the pore close-off (PCO) depth? Do you separate the compaction of deep firn into ice (which changes the PCO depth) from dynamical thickening or thinning of the underlying ice? Where is your height reference point? at the PCO depth? at the bottom of the ice? At sea level?

If it is at PCO or bottom of the ice, then the buoyancy is outside of the system and shouldn't be included. If it is at sea-level, the isostatic rebound, and bedrock movement should also be in the equation.

If you indeed placed yourself in a reference system where isostatic rebound is not important and where buoyancy is, then please define how you calculate buoyancy and ice thickness.

- Section 2.5: Please use the same order as above to introduce your observations: i) Surface snow density, ii) Densification, iii) Surface height

You could even consider moving the observations' description at the end of sections 2.1.1, 2.1.2 and 2.3. So the reader finds out about the data right after it read about the fitting method or the surface height definition.

-l.181 "122 firn cores" I'm guessing this includes the 104 cores used for tuning MO. Please rephrase into "In addition to the dry cores used for the tuning of MO ratios, another 18 cores that could not be considered dry were used to evaluate the model output."

There still 11 profiles missing from the 125 + 8 profiles mentioned in l.174.

- l.183 "Montgomery" Please give the original source/reference of the profiles, along with the SUMup citation.

- l.184 "top 0.5 m" This top layer thickness does not match with the 3 cm thickness of the top layer, nor the 1h time step of the model.

- Figure 1: Here a bit of creativity could help to avoid symbol overlapping each other. For example, z550 and z830 could be shown by half disks, and sensitivity analysis could be shown as a box surrounding the markers.

- l. 197 "105 observations" From the legend in Figure 1 I thought that sensitivity analysis would be only done at the gray dots.

- l.199 "10 additional locations" Now I am concluding that the gray dots are sensitivity analysis locations where you have no density observation available. Please change the marker of the sensitivity analysis to a different one than the observation location and remove the gray fill to show that the marker does not mask anything underneath.

I also see that there are 105 sensitivity analysis location while you use 133 profiles for tuning and evaluation of the model. We need to see which locations have observations but are not part of the sensitivity analysis. I am also unsure how you summarize the sensitivity analysis at these 105 locations into Table 4. Are the results averaged? How do we know that the average is not biased due to over/underrepresentation of certain climate zones in the 105 sites selected?

- l. 204 "-1.5K" Does this evaluate the air temperature or the surface skin temperature (which is that actual input of IMAU-FDM, I assume?)?
- l. 212-213 I thought that you had no data before 1979? what are these years corresponding to?

Please update to something like "To mimic this increase in precipitation, we create a spinup in which model loops three times over the 41-year-long reference period where we decrease precipitation in the first loop by 10%, by 6.66% in the second and by 3.33% in the third". Make something similar for the temperature.

- l.224 Using different symbols and reducing the size of markers, I believe you can display this third comparison in Figure 2a. Please mention that the FDM FS-K shown in Figure 2a are calculated with RACMO2.3p2 surface climate. In IMAU-FDMv1.1, FS-K was used but the surface climate was also different, therefore we still don't know if the fresh snow density in the new model is greater or lower than in IMAU-FDMv1.1.
- l. 234-238 Brils et al. uses yearly temperature because it is the only type of parametrization of surface snow density in Greenland. There has not been any evaluation of the impact of wind speed in Greenland because there is simply no instantaneous and collocated measurements of temperature, wind-speed and surface snow density. In turn, people used yearly temperature, because they thought it was more robust and less model dependent.

Also, a surface snow density parametrization built on simulated T and WS will only be valid when using T and WS from the same model because it accounts for potential model biases in the T and WS values. This should be mentioned somewhere.

Consequently, I don't think that the difference between the parameterizations in Greenland and Antarctica tell anything about the snow or the climate and might be just arbitrary.

- l.241 "the optimal MO is less steep" Please add, at least at the beginning of the paragraph, plain word explanation of what "lower/higher MO830" mean (i.e. overestimated/underestimation modeled densities for the top/deep firn). I'm still not sure if the MO correction enhances or inhibits the densification.

- l. 253 "Differences between" Please mention what are these differences. In terms of MO fits but also in general terms: Is densification faster/slower in than in Greenland or less/more responsive to accumulation or temperature?

- l. 275 "Figure 4d shows the age of firn" This part could be more quantitative. What is the median/maximum firn depth, what is the median/maximum age at PCO? What is the fraction of the ice sheet covered by firn?

Since you mention the age of the firn at PCO and its usefulness for ice core interpretation,

could you present the age of the firn at PCO depth for ice core locations and compare to the values found in literature? Since you are trying to show that the model output can be used for this purpose, you might as well show how well it does.

Otherwise this paragraph and the figure could be removed to leave the focus on the surface height and FAC discussion.

- l.288 "somewhat" Please avoid this word and replace by quantitative information.

- l.289-290 "higher accumulation rates" Can you quantify this difference?

- l.300 "also contributes to the general pattern..." Higher melt could indeed participate to the lower FAC at low elevations. But increased snowfall in the interior does not show in the new FAC pattern (FAC decrease in the center of the ice sheet in Fig5b). This could mean that the MO update overprints the effect of higher accumulation.

You also mention that the fresh snow density is decreased since the last version. This should have enhanced the FAC increase due to increased accumulation.

- l.318 "Vice is constant" Why is it constant by definition? How do you define it?

- l.319 "Vbuoyancy is negligible" I'm still not sure how you define this and why is it negligible?

- l.326 "in the timing" and magnitude?

- l.327-328 "63 to 68%" I am not sure how you calculate this number

- l. 332-335 Isn't it redundant to the discussion of Figure 5c? Could be moved elsewhere or removed for concision. See typo "and"

-l.335-338 Does not add information. Consider removing for concision.

- l.345 "5 to 10 years" Why does it matter that it last 5-10 years? How does that relate to frequency of El-Niño or annular mode?

- l.340 "Above 2000 m ..." I don't fully agree with this sentence. The variability in  $V_{fc}$  is much lower than below 2000masl. "larger temperature variability" than what/where?

- l.352 "sd" Spell out what it stands for the first time you use it .

- l.353 "67%" It sounds like a repetition from l.327-328, but calculated slightly differently. Maybe keep only one of the two formulations.

- l.358-364 This is methods.

- I.366 "Ligtenberg et al. ..." Please present your results first, then bring in previous simulations to put your results in perspective.
- I.368 "likely explained by altimetry errors" This deserves more details. What was the motivation to say that? What altimetry errors was it?
- I.369 "The altimetry observations prior to 2003..." This should be moved to the method and further justified. What is the measurement precision threshold that you use?
- I.374-376 Since you mention that the improvement is due to better melt forcing in the costal margin, can you present the same seasonal surface height amplitudes for the altimetry product and two model runs below and above 2000 masl (or any elevation that is relevant). This would provide the evidence that the improvement comes from these coastal regions.
- I.379 "linear regression" Please give more detail: a linear regression is fitted to...annual/monthly/daily values... for the period ...

- I.385 "excluded" I understand that high dynamical imbalance regions should be masked out of the altimetry product. But there is no reason to remove it from the FDM to FDM comparison in Fig 8e.

- I.385-388 Please merge these two sentences and show that the "17%" is what make you say that there is improvement.

"especially in

Dronning Maud Land, Wilkins Land and Adelie Land, where the FDM v1.2A trend has become either more positive or less negative." is not. The regions are not indicated on any maps and you don't mention which ones became "more positive or more negative", neither why the simulated trend became more positive or more negative in these areas. Either give a proper description or remove .

- I.388-393 Please explicit how you rule out long term trend in ice dynamic thickening?

- I.394-402 After briefly introducing Fig9, you end up discussing residual trends (I. 399). You might as well refer to Fig8d (which should be annotated with the names of the places mentioned in the text).

Eventually in this paragraph you only mention location 5, 1 and 6. Which means that most of Figure 9 is not described in the text.

The only piece of information given by Figure 9 is this variability in the altimetry product. It should be described and discussed better in the previous paragraph, where this variation actually matters. In that paragraph (Section 5.1) please give a metric for the this change in inter-annual variability: please show that on the majority of the ice sheet the

standard deviation of detrended altimetry height change is significantly higher before 2003 than after.

I suggest removing figure 9 and merging this paragraph with the two previous ones.

- I.401 What is the proposed explanation for this increased variability before 2003? Could it be the frequency of the altimeter that was more sensitive to change in penetration into snow?

- I.409 "+/-6%" Is this the temperature uncertainty or the magnitude of the FAC change? See my suggestions for Table 4 to avoid misunderstanding.

- I.409 Can you be more specific than "somewhat increase"?

- I.410 "robust" What do you mean by robust?

I would rather say that the MO fitting allows to produce realistic firn densities even if an arbitrary bias is applied to the forcing.

It is also important to mention here whether the RMSEs showed here are from the measurements that are also used for the fitting of MO or if they are independent. If they are the one the MOs are being fitted to, then it is normal that they remain low after re-fitting the MOs.

- I.411 This shows the limitation of using the entire period as a reference period for spinup.

- I.416 "yields an average surface elevation trend" Does that help to get closer to the altimetry results in some areas?

- Table 4. Please add the magnitude of the change applied in each run (not just the sign).

Are the relative changes in FAC and RMSEs in m or in %?

Is the FAC for all the ice sheet or the average change at the density profile locations?

- I. 417 "linear regression" I am wondering if this linear regression is necessary, since you have access to the model run. Could it be rephrased into "We notice that this trend (+3.3 cm yr<sup>-1</sup>) is more pronounced in high accumulation areas and find that, in this scenario, the average height change of areas with accumulation >1000mm yr<sup>-1</sup> is 5.x m over the 1979-2020 period." ?

Once again, would this scenario get the model closer to the altimetry observations? Do you deem it more realistic than the reference run?

- I.426 "partly explains" Please be quantitative. What was the improvement given by the inclusion of accumulation and temperature trends in the spinup? How much of the observed surface height trend is left unexplained even in this scenario?

- I.430 I'm not sure what you mean by "robust". Please spell out.

- I.431 "error range of altimetry observations" The high uncertainty on the altimetry product (variability before 2003, uncorrected penetration, high relative uncertainty in the interior) poses the question of the suitability of this altimetry dataset for the evaluation of the firn model. There are many other datasets available (coffee can compaction experiments, multi-year gps records, ice bridge flights and as you mention ICESat 1 and 2 which have no penetration issues).

My main concern is that since the only observations presented in the manuscript are either used to tune the model (density profiles) or are too uncertain to truly assess the quality of the model (altimetry), I am wondering what knowledge we gain about the model or the processes it tries to describe. Where is its weakness? Where should it be improved next?

- I.438-442 This is a repetition of what was said before. Could be removed or merged with the conclusion.

- I.463 please replaced "somewhat improved" by a more quantitative "improved by X%"

- I.464 Please spell out what you mean by robust?

- I.465 This last statement is rather loose and arbitrary. You could move your outlooks to the end of the conclusion to finish on future perspectives.

- Code and data availability: Note that the Cryosphere is i favor of an open code and open data science:

"We recommend that any data set used in your manuscript is submitted to a reliable data repository and linked from your manuscript through a DOI. Please see our data policy. Please also consider other assets like software & model code or video supplements." (TC instruction to authors)

I know that it is something that has been done before by your research group and it would be great to continue in that direction.

As a minimum, the scripts to reproduce the analysis and the figures should be made available to reproduce the study's results from the model output.

Please also mention where to find the density profiles that are not in SumUp and the altimetry data.