Review of:
"Characteristics of the contemporary Antarctic firn layer simulated with IMAU-FDM v1.2A (1979-2020)"

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Reviewer: C. Max Stevens

Summary:

This paper presents an updated version of the IMAU firn densification model (FDM) for Antarctica. The IMAU FDM is commonly used by other research groups that need firn model outputs. The changes to the model include an updated formulation for the densification rate, an improved surface density equation, and a different parameterization for the thermal conductivity. The new formulation is tuned using a better RCM forcing than the previous version, and it also leverage more firn core data for tuning. The authors show that in general, the updated model formulation's outputs match surface elevation change measurements better than the previous version. The paper includes an analysis of the sub-annual and interannual variability in surface elevation and its causes. The authors also performed a range of tests to understand the model's sensitivity to input and parameter uncertainties.

In general, I found this paper to be scientifically insightful and well written. Its analysis of surface height variability is timely given recent advances in satellite remote sensing. I think that the paper will be a good addition to *The Cryosphere*. Below I list general and line-by-line comments that should addressed prior to publication.

General comments:

1. The sections (2.6, 6) describing the sensitivity tests need to be clearer – I am still not sure exactly what you did for them. For example, what exactly are you adding and subtracting for those runs? Are you, e.g., adding 8% to the accumulation over the entire forcing, and that is one of the sensitivity runs? It would help to clarify if you wrote exactly what you did to perform these tests, e.g. "We added 8% to the accumulation forcing, and then re-ran our model calibration procedure to get the optimal MO fits (or whatever) with that forcing. We then ran the model with these MO fits and compared the outputs to our baseline model run". (Or something along those lines). I think what I just wrote is the gist of what you are doing, but this is what I need clarified. The distance between sections 2.6 and 6 made this analysis more difficult to understand: I was looking for a table describing the sensitivity tests when I read section 2.6, but did not find it until the end (Table 4). I would also appreciate if table 4 was more specific: e.g., I can figure out what Accumulation+ is from looking at section 6, but it would be nice to have all the information in the table.

   Are you only doing these sensitivity runs for the observational sites, or did you run the model for the entire ice sheet in any of the sensitivity runs? When you say "10 additional locations" (line 198), what are those in addition to? Are those 10 sites special, or were they not used in the original calibration (if so, why not)?

Finally, when doing these sensitivity tests, if you are adjusting the MO fits (section 6), with an empirical firn model, shouldn't you expect the model outputs to match the data reasonably well? This is indeed what you found, but my point is that if you are tuning a firn model using e.g. biased accumulation fields, the firn model will effectively act as a filter for that bias (or, the correction for it will be built into the model), so that the model output will match the data well. I think the upshot of my question is that I would like to see a bit more discussion contextualizing the meaning of the model sensitivity. Is the new model an improved representation of the physics compared to the previous IMAU FDM, or is it just a numerical response based on using different boundary conditions (RACMO forcing and surface density)?

2. In Figure 9 and 5.2: It seems to me that you have taken a rather qualitative approach here in looking at the elevation changes (e.g., "shows comparable patterns", "agreement seems"). I am curious how you have chosen to plot these time series: what is your zero/reference point? To me it would make most sense to plot them all zeroed at the start of the time series (1992) to see a direct comparison of how the data and model differ. The way it is plotted presently is misleading in a few cases: e.g. for site 6 (West Antarctica), the curves appear to be lined up reasonably well, but upon closer examination the data show a clear positive elevation trend while the model predicts a net decrease in elevation. This would enable a more quantitative analysis, such as fitting a trend line to each time series to get the long-term trends. Then, you could de-trend and do a time-series correlation for each site to quantify how well the models predict the shorter time-scale variability.

3. Discussion about implications: My general feeling in the paper what that it was light on discussing the implications of the research in the broader glaciological community. Given how much IMAU-FDM has been used for altimetry studies, I think adding a paragraph or a few sentences describing how the model changes affect our understanding of how the AIS is changing. Do the conclusions drawn by the users of IMAU-FDMv1.1p1 need to be updated?

Line by line comments:
44: Can give a sentence with a broad overview of what the 2 types are for readers not familiar with the difference?

47: "less": change to "fewer"

Table1/section 2.1.1 leaves me wondering what surface density was used for run FDM v1.2A. Perhaps change Table 1 to include a 'Fresh Snow Density' column?

Section 2.1.2: Do you use the instant accumulation rate or 'mean accumulation over the lifetime of the firn layer'? If the former, please provide detail as to why you choose this and deal with the fact that densification will be zero if there is a timestep with zero accumulation.

106/Equation (3): the Arrhenius factor is missing the e. Also (and this is a bit pedantic), $\dot{b}$ in Arthern et al. (2010) has units of kg m^{-2} a^{-1}, which then gives a densification rate $\frac{d\rho}{dt}$ with units of kg m^{-3} a^{-1}. (Arthern defines the units for the factor *D* in their Appendix B). Your accumulation units (mm w.e a^{-1}) are numerically equivalent, but with them the densification rate in your Equation 3 does not end up with units of density per time.

114-122: It is not entirely clear what you mean with MO_{550} and MO_{830}: is MO_{550} the value that should be used for $\rho < 550$, and MO_{830} for $550 < \rho < 830$? Also, perhaps specify that MO added as a multiplier to equation (3).

122: Add a sentence at the end of this section explaining that the parameters are tuned, and this tuning is described in Section 3.

Section 2.2: Do you set the bottom of your model domain to be the depth of the 830 horizon, or does it go deeper? In some locations in Antarctica, there is enough of a temperature gradient at the bottom of the firn to affect the temperature – if you are not modeling through the depth of the ice sheet, do you account for this heat flux?

142: not clear – why is the air content increasing?

160: change semicolon between references to “and”

181: This is confusing – at the beginning of the paragraph you state that you use 125+8 density profiles, but then here you say you used 122. I think that a bit of language clarification in this paragraph will help – something along the lines of, ‘We gathered data from 125 density profiles from firn cores and 8 density profiles from neutron…’. Then, ‘104 of our 133 cores fit the dry-snow criteria needed for our MO-fitting routine.’ Or something along those lines adding a bit more specificity.

182: This sentence has the word ‘density’ 3 times – can you rewrite it to make it a bit easier to read?

184: In some locations, this 0.5 m is likely snow (i.e. less than a year old), but in the interior it is several years of accumulation. Is this a concern, either for tuning or for interpreting model outputs?

203/205 (elsewhere too?): fix the +- to +/- (may be done in manuscript typesetting?)

219: Reference the equations from earlier section.

Table 2: It appears that the letters A,B,C,D do not correspond to equations 1 and 2 (e.g. the Lenaerts equation (2) does not have a D, but Table 2 lists a D). This makes the arguments in section 3.1 difficult to follow (though it appears that C is consistent).

221: The MO fit bit here is distracting – move to end of section. Also, "impact of the different MO fits on the surface snow densities is negligible" – shouldn't there be zero impact, because the surface snow density is not a function of the MO? I.e., changing your surface density scheme will change the MO values after tuning, but changing the MO values will not change the surface density.

Section 3: I assume that the r^2 statistic is calculated by regressing the modeled surface densities against the 1:1 line – is that true?

227: You say: "This aligns with the fact that IMAU-FDM does not include densification by wind packing." I would agree that the subsurface densification scheme does not include wind packing as a densification, but that is also true in reality, isn't it? But, if you are using any of the Kaspers, Lenaerts, or modified Lenaerts equations in IMAU-FDM to set the surface density, I would suggest that wind packing is implicitly included in IMAU-FDM because those equations do include a wind term to account for wind packing.

230: "reduced with": do you mean "reduced by"?

231: You provide a specific number that is the surface density reduction (i.e. 18 kg m^{-3}), which implies that the density was reduced by that everywhere; but then you point to different regions and imply that the density changes are different in those regions. I think you mean something along the lines of, "On average over the AIS, the surface density was reduced by 18 kg m^{-3}. The surface density decreased more in the high accumulation margins and less in windy escarpment regions." However you rephrase it, please be sure to use careful language to indicate what less and more mean, since you are dealing a with a reduction (i.e., does "less" mean reduced by 30 rather than 18, or reduced by 10 instead of 18?).

235: remove comma after v1.2G

237: The idea that the surface density in Greenland is a function of annual temperature seems rather unphysical to me – I think that our knowledge of snow science is adequate to state with confidence that in reality, local conditions on relatively short temporal and spatial scales are the determinates of snow density (which is consistent with what you are finding in Antarctica). I suggest that the discrepancy is probably more due to lack of appropriate data in Greenland, and not significant differences in climatic conditions (there are many locations in Greenland and Antarctica that have very similar climates – so why in Greenland would the density instead be determined by the previous year's climate?).

240: This is inconsistent – the first sentence of the section says you ran IMAU-FDM without MO fits (does this mean with the original Arthern equation?), and then then next sentence talks about the resulting MO fits.

Table 3: Is there an error for your updated α value for MO_{550} FDM v1.2A? It is 1000 times the others listed.

Table 3: Why do you not get RMSE values for the first 3 rows?

243: Do you mean that more densification is needed to match density profile measurements?

246: "When the locations are rerun …" sentence is confusing. Change to something like: "When the locations are rerun using the new MO values with the power fit for MO_{830}, the resulting RMSE of the modelled z830* and z830 (firn thickness) are respectively 25 and 23 % lower compared to the logarithmic fit (Fig. 3c)".

Figure 3: It would be nice on panel (c) if the colors matched the same model run in the other panels (e.g. FDM v1.2A-log should be orange in all cases)

286: change to: 'contain the most air' and 'along parts of the coast'

287: change to 'areas with very high accumulation'

Figure 5: the high values on panels (c) and (d) are one the peninsula, which somewhat removes structure from much of the ice sheet. Consider changing the color bars to max out at a lower value (e.g. 2 m for panel c) and use an extended colorbar (as you do in panel b).

315: You say that snowfall is highest in winter, but I think that is only true in certain parts of the ice sheet.

325: I am confused here: you say that the average seasonal amplitudes are defined as half the peak-to-peak values of the average seasonal cycle (and for FAC that is 2.4 cm). Then, you say that the average seasonal peak-to-peak values for FAC is 8.5 cm. I am not clear why, by your definition, the average season peak-to-peak is not just double (i.e. 4.8 cm) the average seasonal amplitudes. I am guessing that I am just not interpreting this correctly, so perhaps you can change the language to make it clearer.

328: Change the word 'It' (starting the sentence) to 'This result' or similar ("It" acting as a pronoun does not have specific noun it is referring to).

Figure 6d/335: It might be simpler to just say 'average day of year of maximum firn height' rather than "phase".

Figure 7: I am not sure of the best solution, but I think this figure is trying to convey too much information. It is rather difficult to look at the behavior of 7 lines. Additionally, I think it would be nice to pick a representative ~2 year period to plot with months marked to be able to see the seasonality (which you do a good job of discussing in the text) more clearly.

382: Figure 8 caption specifies that 8d is model minus altimetry, but this is not as clear in the text. Consider change to: ‘which is calculated by subtracting the altimetry elevation change from the FDMv1.2A elevation change’.

383: Likewise, what is your subtraction for Figure 8e? FDMv1.2A minus FDMv1.1p1, or vice versa? Am I interpreting this correctly that blues are where FDMv1.2A is improved vis-à-vis FDMv1.1p1? I am a bit confused on 386-7 where you mention “less negative” – any negative (red) area in 8e is where FDMv1.1p1 is better than FDMv1.2A, correct? (Because you are subtracting the absolute residuals). So, to what are you referring that demonstrates the less negative?

389: trends → trend