

Response to Reviewer 1:

We would like to thank dr. Vincent Verjans again for thoroughly reviewing this study. Based on these comments, we have clarified the text and added new figures to the supplementary material. Responses to the comments of the reviewer are written in blue and citations of the manuscript in green.

Kind regards,

Sanne Veldhuijsen

Minor comment 1)

Section 6 still needs some work, and the last two paragraphs in particular. I have read this section several times, and I still do not understand how the statements presented can be derived from the results. I specify these statements here:

-(1516-517) "In non-spin-up experiments, the uncertainty is only 4 % of the total surface elevation change." Is this 4% value calculated at the end of the time series? Or averaged over the time series? Or something else?

There was a typo in this sentence, that explains the confusion, we have improved as: "The uncertainty of the non-spin-up experiments (Fig. S7b) is only 4 % of the total surface elevation change uncertainty (Fig. S7a)."

-(1517) "This uncertainty is smaller than the 40 % that can be derived from Table 3." I do not understand how this can be derived from Table 5 (note that the Table number is wrong in the text). Can the authors please clarify how they calculate the 40% value? Indeed this should be Table 5. See comment above.

-(1524-525) "It indicates that imposing an uncertainty in the experiments mostly has an impact on the high density, low-FAC part of the firm column." I understand that the uncertainty in FAC is lower than in dH/dt. However, I disagree that this necessarily implies that mostly high-density firm is affected by uncertainties here. For example, density could be higher at all vertical levels (due to higher temperatures) but compensated by a larger total firm column thickness (due to higher accumulation). This can result in similar FAC values (i.e., low FAC uncertainty) but with density values affected throughout the firm column. Therefore, the authors should support this statement quantitatively, and using extra figures in the supplements that show that low-density firm is unaffected by the sensitivity tests. When analyzing the results, we indeed saw that also the low-density part of the firm is impacted. Therefore, we have decided to remove this sentence. However, in very high-accumulation regions, a large part of the firm does not originate from the spin-up period, therefore here its main impact is logically on deeper, high-density firm. Therefore we have reformulated the sentence: "We attribute this to the fact that in high-accumulation regions, a large part of the firm layer does not originate from the spin-up period. Here, the main impact is on deeper high-density firm, meaning that the impact on a volume to mass conversion is limited compared to the large uncertainty in surface elevation change."

-(1525-526) "The spatial pattern in FAC change uncertainty for the spin-up experiments is different from that of surface elevation uncertainty (Fig. S6)" This is not clear to me when I compare Fig. 10b and Fig. S6. In contrast, I find the patterns generally similar. Thank you for noticing. They are indeed generally similar, especially when looking at the figures. However, as we discuss there is a decreasing uncertainty for high-accumulation regions (>600 mm/yr).

We propose to rewrite this:

"The spatial pattern in FAC change uncertainty for the spin-up experiments is in general similar to the surface elevation change uncertainty, apart from a decreasing uncertainty in high-accumulation regions (>600 mm/yr)."

-(1527-528) "We attribute this to the fact that in high-accumulation regions, the imposed uncertainty has an effect on the entire firm layer much more quickly." Why is that? Can the authors clarify how they reach this conclusion? What we mean is that in high-accumulation regions a large part of the firm does not originate from the spin-up period. We propose to clarify this as follows: "We attribute this to the fact that in high-accumulation regions, a large part of the firm layer does not originate from the spin-up period."

-(1533-534) "It suggests that the ice-sheet-wide uncertainty due to uncertainty in the spin-up climate is lower than suggested in Figure S7c." Do the authors mean that their uncertainty estimates provided in Table 2 overestimate the actual uncertainty? In this case, this contradicts the references that are provided in Table 2.

Some locations experienced an increase in accumulation whereas others experienced a decrease in accumulation over the last centuries. This has opposite effects on the surface elevation change, and therefore the actual uncertainty averaged over the AIS is lower than expected. However, due to poor spatial coverage of accumulation records over the AIS, it is not feasible to perform separate experiments for all regions. In some regions, no significant trends have been observed, but on average we estimate an uncertainty of -5/+5%. Therefore, we rephrase as follows:

"Over the remainder of the AIS, some locations experienced an increase in accumulation whereas others experienced a decrease in accumulation over the last centuries (Thomas et al. 2017). This has opposite effects on the surface elevation change, which suggests that the surface elevation and FAC change uncertainties averaged over the AIS are likely lower than the results of the spin-up temperature and accumulation uncertainty experiments (Fig. S7c).

Specific line-by-line comments

-Title I suggest: "Characteristics of the 1979-2020 Antarctic firm layer simulated with IMAU-FDM v1.2A" Thank you for this recommendation, we have adjusted the title accordingly.

- 12 Here and elsewhere in the manuscript, I suggest replacing “ice thickness” by “ice mass”. [Done](#).
- 16-7 “seasonal and decadal surface height variability is due to variations”: replace is by are. [Done](#), thank you for noticing.
- 17 Add: “firn mass, respectively”. To clarify, we changed this to: “We found that 62 % of the seasonal and 67 % of the decadal surface height variability are due to variations in firn air content rather than firn mass.” (there was also an error in the values mentioned in the abstract/conclusion). We also changed this sentence in the conclusion.
- 134-35 Replace: “in mass- and density-change components”. [Done](#)
- 135 Correct: “an ice-dynamical”. [Thank you for pointing this out. Done](#)
- 137 Add comma: “climate, enhanced” [Done](#).
- 152 Replace “while” by “but”. [Done](#).
- 172 Replace: “characteristics”. [Done](#).
- Table 1 Include FDM v1.2G. [We included FDM v1.2G in Table 1 of the revised MS.](#)
- 1108 Replace “depositing” by “deposition”. [Done](#).
- 1113 Please specify: “to simulate the fresh snow density, here applied to the top-most model layer (typically xx cm thick)” OR “to simulate the fresh snow density, here applied to the top 0.5 m of the model domain”, depending on the modeling procedure. The fresh snow density, is the density of het fresh snow that is added on top, and not of the top most layer or 0.5 m of the model domain. We propose to clarify as: “These fit coefficients are then used to simulate the fresh snow density, i.e. the density of the fresh snow that is added on top of the firn column.”
- 1114 Replace "include" by “perform”. [Done](#).
- 1123 Replace "measure" by “proxy”. [Done](#).
- 1124 Replace "assuming that the accumulation rate is constant" by “approximating the accumulation rate as constant”. [We agree, done](#).
- 1128 Replace “by deformation, sublimation and diffusion” by “by deformation, recrystallization and molecular diffusion”. [Thank you, done](#).
- 1141 Replace "include" by “perform”. [Done](#).
- 1151 Is it only retained meltwater that is allowed to refreeze? I believe that meltwater reaching a subfreezing layer is allowed to refreeze even if it is not retained through the irreducible water content before. [That is indeed true. We propose to change this into: “The meltwater refreezes when it reaches a layer with a temperature below the freezing point.”](#)
- 1187 Replace “retuned cloud scheme and snow properties” by “retuned schemes for clouds and for snow properties”. [This was a typo and is now fixed as follows: “RACMO2.3p2 employs upper-air relaxation, has updated topography, and a retuned cloud scheme and modified snow properties.”](#)
- Section 2.5 Is it possible to describe better how the separation between calibration and evaluation cores was decided? [This was a pragmatic decision, as some were added later when calibration and computational demanding simulations of the entire AIS were already performed. This is clarified in the MS.](#)
- 1214 I think that “method and citation” should be plural. [This has been improved.](#)
- 1221 Add comma: “15 cm, depending”. [Done](#).
- 1230 Typo: “is currently too short”. [Thank you. Done](#).
- 1242 “excluding five sites that were later added for evaluation”: this is not clear. Is it meant that five additional sites were later added for evaluation? [See our response to Comment 1 of Reviewer 2.](#)
- 1247 Replace “a model sensitivity test, which includes” by “a range of model sensitivity tests, which include”. [Done](#).
- 1248 Is the fresh snow density varied independently at each time step? [No, it is adjusted with a constant value. To clarify this, we have rephrased this as: “The fresh snow density was uniformly varied with the RMSE from the evaluation with in situ measurements \(+/-30 kg/m3\)”.](#)

- 1252-254 Similar question: are the temperature variations applied independently at each time step? Similar as above. We specify this as: “This spread varies spatially and is assumed to be constant through time.”
- 1257 Change: “isotope”. Done.
- 1258-259 A citation is needed for this statement. We agree and added Stenni et al. 2017 and Thomas et al. 2017.
- 1259 Replace “about typically” by “approximately”. Done.
- 1268 Please specify “we also performed two tests”. Done.
- 1272 Replace “a steady-state firn layer” by “a firn layer in steady-state”. Done.
- 1272 Replace “this simulation” by “these simulations”. Done.
- 1274 Specify: “and/or temperature”. Done.
- 1275 Why “randomly”? Please clarify. Over the period 1979-2020 there are no remaining trends in surface elevation due to the assumption of a steady-state firn layer, but sub-periods can also have limited trends. In 2015-2020 we did find trends in surface elevation change for most locations, which enables us to test the sensitivity. We propose to change this into: “In the remainder of the sensitivity experiments, we therefore compare surface elevation change over the period 2015-2020, as most locations experienced clear trends in surface elevation over that period.”
- 1278 Replace “included” by “shown”. Done.
- 1287 Rephrase: “extent (bias = 10.3 kg m⁻³). It is the update in the atmospheric forcing that causes a poorer (...)”. Thank you for this recommendation. We have changed this accordingly.
- 1296 If this is correct, please specify: “is reduced by 18 kg m⁻³ on average”. Done.
- 1299-301 In my view, these two sentences can be removed. We have added these sentences following a comment from the Editor. Therefore, we decided to keep the first sentence. However, we removed the last sentence, as this is already mentioned.
- 1302-303 How can do the MO values influence the rho0 parameterization? Is it not the other way around? The surface snow density, is the density of the top 0.5 m. Some densification has taken place, and is thus influenced by the MO fits.
- Table 3 The R2 values do not agree with the R2 values of Fig. 2. Thank you for noticing this. This has been corrected.
- Table 4 First, MO coefficients have no units, and thus the last column should be unitless. Second, RMSE should be replaced by RMSD (root mean squared deviation) because this quantifies the deviations with respect to the fitted function, and not errors. Finally, RMSD values should be given in all rows. This should indeed be unitless and RMSD, we have changed this. Unfortunately, we do not have the required data of FDM v1.1 to calculate the RMSD of its MO fits. We added the RMSD of FDM v1.2A for MO500, which is 0.13. Furthermore, the r2 values for both FDM v1.1p1 were taken from Ligtenberg et al. 2011, hence using a different and smaller observational dataset, and not the observations dataset used for v1.2a. As this is not yet clear in the manuscript, we added the following sentence to the caption of Table 4: “... et al (2011). Please note that R2 for v1.1a applies to the calibration dataset used in that study.”
- 1316 The comparison with FDM v1.1p1 and FDM v1.1p2 is unfair. These models were not calibrated with the data of this study, whereas FDM v1.2A was calibrated with (almost) all the data used for evaluation. This must be specified. Therefore we also compare to FDM v1.2A-log. In order to further mitigate this unequal comparison, we have now derived these percentages using only the FAC observations that were used to calibrate FDM v1.1p1 and FDM v1.1p2, and wet cores that were not used for evaluation at all. We updated the manuscript accordingly: “When we only include the seven observations that were used to calibrate FDM v1.1 and, two wet firn cores, the RMSE of FAC simulated by FDM v1.2a is 2.79 m, which is 19.5 % and 9.0 % lower compared to FDM v1.1p1 and FDM v1.1p2 for these observations.”
- 1324 Correct: “asymptotically”. See comment below.
- 1324 To avoid any confusion, please specify: “asymptotically towards zero, but less rapidly than FDM v1.2A-log”. We clarify: “where the MO fit curve decreases less rapidly than FDM v1.2A-log.”
- 1330 Replace “68” by “0.68”. Done.
- 1348-349 Please quantify: “ The patterns vary spatially across climatic regions with temperature as a primary driver and accumulation and surface melt as secondary drivers.” As this is hard to quantify, we decided to not differentiate between primary/secondary drivers. This has been formulated as: “The patterns vary spatially across climatic regions with temperature, accumulation and surface melt as drivers.”

- 1353 Please specify: “in younger, thus less densified”. Thank you for this recommendation, we have changed this accordingly.
- 1360 “If we compare this to observations”: which observations give firm age? We have specified this: “When comparing this to firm age inferred from $\delta^{15}\text{N}$ measurements for 15 locations (Breant et al. 2017), we find ...”
- 1360-361 “On average, the RMSE is 25 %”: is this really the RMSE (i.e., the mean across all the errors) or rather the average ratio between the individual absolute errors and their corresponding observed value? This is indeed the ratio of the individual absolute errors. We have specified this : “On average, the absolute errors are 25 % of the observed values.”
- 1361 “In comparison, RMSE of z830 is 15 % of the observed values”: same question as comment just above. See comment above. We have specified this: “In comparison, the absolute errors of z830 are 15 % of the observed values on average, and $R^2 = 0.73$.”
- 1366 Replace “would be” by “was”. We think this should be “would be”.
- 1371 Specify: “Community Firm Model”. We have specified this.
- 1375 Replace both instances of “new” by “updated”. We agree. Done.
- 1378 I think that “FDMv1.2A” should be replaced by “FDMv1.1”. That is indeed true. Done.
- 1383 Correct: “contribute”. Done.
- 1387 Correct: “values”. Thank you, done.
- Section 4.3 Is it possible to provide maps of mean accumulation, melt, and sublimation in the Supplementary Information? We agree that this is a good idea. We have provided maps of mean accumulation, melt and sublimation in Figure S9. We refer to this in Section 4.3 as: “Figure 6c shows that the seasonal firm thickness amplitude can be up to 3 m in the western Antarctic Peninsula, caused by a rare combination of high snowfall and strong melt. Maps of mean annual precipitation, melt and sublimation are shown in Figure S9.”
- 1415 “ as the annual average accumulation is low”: I do not understand why this causes low seasonal amplitude. For a similar seasonality in climatic conditions, the amplitude is lower when accumulation is lower. Low accumulation generally also means low absolute seasonality in accumulation, as zero accumulation remains a hard lower boundary.
- 1421 Replace “its components” by “the components of the latter”. In response to a comment by Reviewer 3, we propose to change this: “Figure 7 shows time series of integrated FAC, the cumulative surface temperature anomalies (against the long-term mean) and the vertical firm surface velocity anomalies (against the long-term mean) for the entire AIS (Fig. 7a) and above/below 2,000 m a.s.l. (Fig. 7b,c). In addition, the surface velocity anomalies are broken down into their components (Eq. 6).”
- 1424 Replace “long time scale” by “slower response timescale”. Done.
- 1426 Typo: vfc is all in subscript. Thank you for noticing, we have corrected this.
- 1430-432 Note that this difference is also explained by the high sensitivity of firm compaction to temperature variability for firm at low temperatures. Thank you for this recommendation, we have added this explanation: “This difference can partly be explained by the larger interannual temperature variability above 2,000 m a.s.l (sd in annual means = 0.78 K compared to 0.48 K), the high sensitivity of firm compaction to temperature variability for firm at low temperatures, and the absence of melt.”
- 1432 Correct: “variabilities”. Done.
- 1435 Specify: “total surface elevation change variability”. Thank you, done.
- 1451 Please remove “which compares well with the FDM seasonal amplitudes”, and replace it by a new sentence: “This suggests that our results of AIS-wide seasonal amplitude lie within the range of observational uncertainty.” Thank you, we have changed this accordingly.
- 1480 Remove “relatively”. Done.
- 1482: Typo: “reduced”. Done.
- 1484-485 Break the sentence: “depth (Schröder et al., 2019). On the other hand,”. We have changed this.

- 1487 vs. 1490 Make sure to be consistent in using brackets or not around the location numbers. [Done](#).
- 1495 If this is correct, specify: “are on average 41% lower”. [This is correct, so we have specified this.](#)
- 1497-498 Replace “is needed” by “we used”. [We agree and have changed this.](#)
- 1498 Refer to Fig. S3. [We assume the reviewer means Fig. S1. We include this reference in the revised MS.](#)
- 1505 Typo: “a surface elevation”. [Corrected.](#)
- 1506 Typo: “of of”. [Corrected.](#)
- 1507 I do not understand “0.7/0.8”. [This should be “0.7”. This has been corrected.](#)
- Table 5 Specify periods in the caption: 2015-2020 for dH/dt, and averaged over 1979-2020 for FAC I think. [The surface elevation change uncertainty for the spin-up experiments is 1979-2020, therefore we decided to only specify this as: “The FAC uncertainty for all experiments and surface elevation change uncertainty for the spin-up experiments are calculated over 1979-2020, and the surface elevation change uncertainty for the MO fits, \$\rho_s\$, \$T_s\$ and b experiments are calculated over 2015-2020.”](#)
- Figure 10 Specify periods in the caption: 2015-2020 for dH/dt, and averaged over 1979-2020 for FAC I think. [See our response to the comment above.](#)
- 1517 Replace “Table 3” by “Table 5”. [Done.](#)
- 1518 Replace “amplifying” by “compensating”. [See our response to Minor comment 1.](#)
- 1519 Replace “Table 3” by “Table 5”. [Done.](#)
- 1522 Replace this sentence by “We expand ice-sheet-wide averaged estimates of uncertainty into time series, show in Figure S7c.” [In response to Rewiever 3, we propose to clarify this, by rephrasing as: “This enables us to expand ice-sheet-wide estimates of uncertainty of the spin-up experiments into time series \(Fig. S7c\), by adding this known constant uncertainty for each timestep.”](#)
- 1529-530 Replace here: “The finding that imposing an uncertainty in spin-up climate has low impact on FAC uncertainty”. (see my Minor comment). [See our response to minor comment.](#)
- 1537 Replace “minus the spin-up sensitivity test” by “minus the transient run spun-up with the sensitivity test”. [We reformulated as: “We found that the absolute residual trend of altimetry minus the spin-up sensitivity test was reduced by 25 %”.](#)
- 1539 “we assume the underlying ice to have responded to the changing climatic conditions”: does this mean that advection if ice at the lower boundary has changed? And is this equivalent to saying that v_{ice} cannot be assumed constant? [Yes, indeed. We have added this explanation: “This suggests that the uncertainty for those locations \(age < 42 years\) is overestimated, as \$v_{ice}\$ can not be assumed constant.”](#)
- Section 7 I would appreciate adding a couple of sentences about larger uncertainties in melt-affected areas. Can the authors also provide the quantitative results of the evaluation at the 10 wet cores? [We found at the 11 wet firm core sites that the RMSE of \$z_{550}\$ amounts to 3.17 m, of \$z_{830}\$ to 8.87 m and of FAC to 4.40 m. We mention this in Section 3.2 Dry snow densification rate, as follows: “For the 11 wet firm cores we found that the RMSE of modelled \$z_{550}\$, \$z_{830}\$, FAC amounts to 3.17 m, 14.34 m and 4.40 m, respectively, and thus larger than the RMSEs of the entire firm core dataset \(2.33, 8.87 and 2.63 m, respectively\).”](#)
[In Section 7 we discuss this as a separate final paragraph: “The densification scheme used in FDM v1.2A \(eq. 3\) is developed for dry snow densification. As it is used for both wet and dry locations, we assume that the densification rate of dry firm is equal to that of wet firm. Since the presence of liquid water impacts the evolution of grain size and shape, this may also impact the densification rate of firm, however due to a lack of physical understanding and available measurements, this has not been included in FDM v1.2A. The RMSE of the modelled \$z_{550}\$, \$z_{830}\$ and FAC for the wet firm cores are respectively, 3.17 m, 14.34 m and 4.40 m, and thus larger than the RMSEs of the entire firm core dataset \(2.33, 8.87 and 2.63 m, respectively\). Uncertainties in the melt forcing \(Carter et al. 2022\) impact the simulated density profile, which may contribute to the lower agreement and hampers this comparison.”](#)
- 1543 Specify: “has been used over the AIS”. [Done.](#)
- 1544 Correct format of citation. [This has been corrected.](#)
- 1546 Specify: “the mass change associated with surface processes”. [This is about total mass change. See our response to comment about 1546 of Reviewer 3.](#)

- 1548 Correct: “updated climatic forcing, as is shown”. [Done](#).
- 1553: Specify: “compared to the error range of the altimetry observations and model uncertainties”. [Done](#).
- 1558 Replace “decadal” by “inter-annual”. [Done](#).
- 1561 Remove comma after “factor”. [Done](#).
- 1565 Replace: “not recommended in combination”. [Done](#).
- 1577-578 See my comment on I316. [We added an additional evaluation of only firn cores that were used for FDM v1.1](#) Because FDM is a semi-empirical model, additional fit data will improve the model by definition, which is also shown by the similar value of 21% RMSE lowering from the entire firn core dataset to 19.5% RMSE lowering of the FDM v1.1 firn core dataset.
- 1582 Phrasing: “as a primary, and accumulation and surface melt as secondary drivers.” [Not relevant in the revised MS](#).
- 1585 Replace “As variations in FAC and firn thickness have a similar phase, 63 to 68 %” by “Variations in FAC and firn thickness have a similar phase, and 63 to 68 %”. [Done](#).
- 1591 Replace “which violates” by “violating”. [Done](#).
- Caption of Figure S4 (h) should be for the accumulation spin-up experiments. [Thank you, this has been corrected](#).
- Caption of Figure S5 (h) should be for the accumulation spin-up experiments. [Done](#).
- Caption of Figure S7 Specify: “the estimated ice-sheet-wide averaged surface elevation change”. [Done](#).

Response to Reviewer 2:

We would like to thank dr. Baptiste Vandecrux again for reviewing and editing this study. Based on your comments, we have clarified the text and Figure 2a. Responses to the comments of the reviewer are written in blue and citations of the manuscript in green.

Kind regards,

Sanne Veldhuijsen

Comments:

line 241-242: "(the firm firm cores used ...)". I recommend removing this sentence as it is ambiguous: did you mean "the firm cores used in the sensitivity analysis"? If yes, then that already makes sense and the sentence between brackets can be removed. If it meant "the 103 cores used to fit/evaluate MOs and z550/850 are located in 100 different model cells and you use only 95 of these in the sensitivity analysis" then the original sentence is very elusive and maybe not so important. **We propose to clarify this as follows:** "These locations include 95 locations from firm core sites (the firm cores initially used for MO calibration and z550 and z830 evaluation are located at 95 unique grid points)."

l. 270 "except for the MO fits" maybe spell out to "except for the experiment where we perturbate the coefficients of the MO fits". Also consider changing "fits" to "function" or something more specific. **Thank you, we have spelled this out according to your feedback. We have not changed fit into function.**

l.275 "randomly selected period 2015-2020", why was that period chosen randomly? you could have used the altimetry period for which you have a separate estimation for the surface height trend? Maybe I missed the explanation, but "randomly" still raises some questions. **See our response to comment l275 of Reviewer 1.**

Figure 2. Thanks for updating it and adding more FDM FS-L and FDM v1.1p1 points. It allows the readers to see, among other details, the negative bias of FDM FS-L. Now it has more overlapping dots. You can consider using different symbols for different runs and using semi-transparency to see overlapping points. This is very optional. **Thank you, we made the inside of the circles transparent to improve the readability.**

l.360 "to observations" give number and reference. **Thank you for this recommendation, we have specified this as:** "When comparing this to firm age inferred from $\delta^{15}\text{N}$ measurements for 15 locations (Bréant et al., 2017), we find ..."

L.482 "recued" reduced? **This has been corrected.**

L.514-523 This paragraph is very hard to read. Please add short explanation (or reminder) of the different quantities you are discussing:

"The non spin up experiments" please give their name as presented in table 5. **We specify the experiments now as follows:** "for the non-spin-up experiments, i.e. the MOfits, fresh snow density, temperature and accumulation experiments."

"is only 4%" please consider spelling out where that number comes from, like: "for these experiments, the uncertainty in dhdt (x-y mmyr⁻¹, table 5) , are only 4% of the total elevation change (z mmyr⁻¹ over period Y1-Y2)". **There was a typo in this sentence, that explains the confusion, we have improved this:** "The uncertainty of the non-spin-up experiments (Fig. S7b) is only 4 % of the total surface elevation change uncertainty (Figs. S7a)."

"40% that can be derived from table 3" give quick reminder of how these numbers were calculated. **See comments above.**

"amplifying effect" what is amplified? I understood from the previous sentence that the sensitivity analysis presented in table 5 ends up to lower uncertainty than first assumed from table 3. **With amplifying we mean that an uncertainty (e.g. lower fresh snow density) amplifies the rate of surface height lowering as well as a surface height increasing. To clarify we propose to change this into:** "This is because an imposed uncertainty has opposite effects across the AIS and in time. For example, lowering the fresh snow density increases the rate of a surface elevation increase/decrease."

"lowering the fresh snow density increases surface elevation change rates of both signs" I am not sure how to interpret this sentence:

i) wasn't the experiment about adding either subtracting 30 kgm⁻³ (l.249) so why only talking about the decreased case? Does the increased fresh snow density also lead to "surface height changes of both sings". I know that it is an example but I don't understand what it illustrates.

ii) I am also unsure what "of both signs" means: do you mean that eventually the changes caused by perturbing the fresh snow density cancel out and that is why the new uncertainty is "only 4%" of the overall trend? Please write out your reasoning so that there is nothing left to guess. **See comment above.**

L. 522. Does "this" refer to the last preceding sentence or to the whole sensitivity analysis? If it is the former, please spell out why or how. **This indeed refers to the last preceding sentence. We propose to clarify this, by rephrasing this as:** "This enables us to expand ice-sheet-wide estimates of uncertainty of the spin-up experiments into time series (Fig. S7c), by adding this known constant uncertainty for each timestep."

L.534 "lower than suggested by figure S7c" please spell out what is in figure S7c (the uncertainty of surface height and FAC due to alternative temperature and accumulation during spin-up), so that the sentence can be understood without going back and forth between the supplementary material and the main text. To clarify, we propose to change this into: “, which suggests that the surface elevation and FAC change uncertainties averaged over the AIS are likely lower than the results of the spin-up temperature and accumulation uncertainty experiments (Fig. S7c).

Response to Reviewer 3:

We would like to thank dr. Max Stevens again for reviewing and editing this study. Based on your comments, we have further clarified the text, by amongst others, specifying how we use the term AIS. Responses to the comments of the reviewer are written in blue and citations of the manuscript in green.

Kind regards,

Sanne Veldhuijsen

Main comment:

The largest issue that must be addressed is the definition of the “AIS” (Apologies to the authors that I did not catch this in the first review). I think the authors are using the term ‘AIS’ as shorthand for the entire continent, including both the ice sheets (WAIS and EAIS), the ice shelves, and distinct glaciers and ice fields on the Antarctic Peninsula. Strictly, the term AIS refers only to the ice sheets, and the authors define AIS as the Antarctic Ice Sheet in the first sentence of the paper. But, then throughout the paper it seems that the analyses are including ice shelves and the peninsula. This leads to some confusion in the paper. For example, the authors mention on line 344 that 99% of the AIS has a firn layer. Is this figure strictly for the ice sheets, or is the peninsula, which contains distinct glaciers, included in the 99%? How about the ice shelves? I think it would be good to specifically define the area(s) included in the study. For example, “We use the term AIS broadly to include the East and West Antarctic Ice sheets, as well as distinct glaciers and ice fields on the continent and the ice shelves”. Thank you for noticing this. We use the term AIS to broadly include the East Antarctic, West Antarctic and Antarctic Peninsula ice sheets, as well as disconnected glaciers and ice caps on the continent and ice shelves, and we agree that it is important to specify this. We propose to add this definition at the end of the Section 2.1 IMAU-FDM as follows: “The model is applied to the AIS, and we use this term to broadly include the East Antarctic, West Antarctic and Antarctic Peninsula ice sheets, as well as the ice shelves and disconnected glaciers and ice caps on the continent.”

Line by line comments:

13: is losing → has been losing. Done.

37: comma after climate. Thank you, done.

63: change ‘this study’ to ‘that study’ to clarify that you are not talking about your own paper. We agree, this has been changed.

130: consider specifying ‘requires dimensionless correction terms’. We have specified this accordingly.

Section 2.3: Consider mentioning in this section that v_{bedrock} also contributes to ice-sheet elevation change but is not included in IMAU-FDM. This section only describes the impact of firn and SMB processes; therefore, we decided not to include dynamical change of the ice and bedrock in the bedrock. However, we do discuss the bedrock contribution in Section 1 (Introduction) and Section 2.5 (Observational data). To improve clarity, we repeat this in this section: “The contributions of ice-dynamical imbalance and bedrock motion to changes in the surface elevation are not included in the current model.”

305: clarify ‘without MO corrections’ – does that mean that you just used the Arthern et al. (2010) equation, i.e. Equation 3 in your paper? Or that you ran the model using the updated RACMO forcing but the MO values from Ligtenberg et al. 2011? (Table 4 does not make it clearer to me). Yes indeed, we just use the Arthern et al. (2010) equation. To clarify this, we have changed this into: “To calibrate the dry snow densification rate we first performed a simulation of FDM v1.2A without MO corrections, thus only using Eq. 3.”

315/Table 4: I got a bit confused here because you refer to FDM v1.1p2, which is not listed in Table 4. From Table 1, I gather that v1.1p2 is the model using RAMCO2.3p2 and the Ligtenberg MO values. To clarify this section, it may be as simple as altering the first and third lines of Table 4 to read, “MO550FDM v1.1p1/v1.1p2” and adding a second R2 value. Then, add a note in the caption explaining the notation and reminding the reader that v1.1p2 uses the RACMO2.3.p2. Alternatively, you could add 2 more lines to the table. As a reader, I was alternating between the text and Table 4 while reading this section, and so I was a bit thrown off when I had to go searching to remind me what v1.1p2 was. I realize there is a risk of redundancy (ie., you did define v1.1p2 earlier), but given the number of model variations that are included in your study I as a reader find it helpful to have reminders throughout the text of what the variations are. We do not have the fit statistics of FDM v1.1p2, since we use the fit coefficients from the Ligtenberg et al. (2011) calibration. Also, there is not much added value, since FDM v1.1p2 uses fit coefficients obtained using a different forcing. Therefore, in Table 4 we only show the statistics of the versions in which the forcing matches that used to obtain the calibration parameters.

To clarify the text, we propose to change this into: “The update in atmospheric forcing without updated MO calibration does not result in an improvement in FAC (shown by the similar RMSE of FDM v1.1p1 and FDM v1.1p2, which is FDM v1.1 forced with the updated forcing RACMO2.3p2).”

325: Similar to my last 2 comments – I am again confused by your naming scheme. Which is the ‘reference FDM v1.2A’? That is not included in Table 1 or Table 4. If one of the runs you are referring to is the -log run, refer to it as you have defined it previously. With reference FDM v1.2A, we mean the ones not from the 10-fold cross evaluation simulations. To avoid confusing, we removed “the reference”.

329: Specify: “if we only include Antarctic cores from the...” Thank you, done.

329: change to, “we find similar model fits to the data for the logarithmic model ($R^2 = 0.67$) and the power model ($R^2 = 0.68$).”

Thank you for this recommendation, we have changed this accordingly.

330: Change to ‘Another difference between FDM v1.2G and v1.2A is that in ...’ Done.

337: Coming from the previous section that discussed many model variations, it would be useful here to specify, “Using the improved firm model FDM v.1.2A, we simulate...” This will make it very clear to the reader what to refer to in Tables 1 and 4. Done.

344: specify: “In line with observations, FDM v1.2A predicts that nearly all...” Done.

352: remove word ‘especially’ Done.

361-362: The sentence about the large spread of timescales in the firm is vague – either add more detail of how these add to our understanding or remove this sentence. I think the spread in firm ages in Antarctica is reasonably well documented in the ice-core literature. I am not sure if you are implying that your findings help with process understanding (e.g., processes operating on different timescales) or implying that your statistics (25% vs 15% RMSE for firm age and depth) can provide new insights – if so, what are those? We decided to remove this sentence.

371: Define CFM; please cite We have specified: The Community Firm Model.

373: Related to comment above – make sure each instance of referring to AIS is consistent or explained. Here – did the values in the previous paragraph not include ice shelves? Based on your previous comment, we decided to specify how we use the term AIS in section 2.1. In line with how we define the term there, we change this : “the entire AIS (including the ice shelves)” in line 373 into: “the AIS” to avoid confusion.

376: spatial → spatially Done, thank you.

420: This sentence needs punctuation and/or to be broken into multiple sentences to clarify. Also consider referring to figure panels in the sentence. Something like: Figure 7 shows time series of the entire AIS (7a) and above/below 2000 m (7b/7c). Each panel includes integrated FAC and the cumulative surface temperature and surface velocity anomalies. The surface velocity anomalies are broken down into their components. To clarify, we propose to change this into: “Figure 7 shows time series of integrated FAC, the cumulative surface temperature anomalies (against the long-term mean) and the vertical firm surface velocity anomalies (against the long-term mean) for the entire AIS (Fig. 7a) and above/below 2,000 m a.s.l. (Fig. 7b,c). In addition, the surface velocity anomalies are broken down into their components (Eq. 6).”

487: change to plural: “find substantial positive residual-altimetry trends at locations” Thank you, done.

494: updated MO fits – perhaps change language in this section to refer to the model versions, e.g. v1.2A, v1.1p2, etc. Then the reader can refer directly to tables to remember which one you are referring to. This is all FDM v1.2A, but for each sensitivity analysis, we also re-estimated the MO fit coefficients now that we changed fresh snow density, accumulation or melt. This is also explained in Section 2.6: “For all our experiments, except for the MO fits, we re-ran our model calibration procedure to get the optimal MO fits. We then ran the model with these updated MO fits and compared the outputs to the FDM v1.2A run.” To clarify this in Section 6, we propose to change this into: “For the sample locations, the sensitivities with updated MO fits are lower than with the FDMv1.2A MO fits (i.e. without updated MO fits), especially the FAC uncertainties, which are 41 % lower.”

518: ‘increases the surface elevation change rates of both signs’ – please clarify what you mean here. To clarify, we change this into: “For example, lowering the fresh snow density increases the rate of surface elevation increase/decrease.”

546: clarify that this is the mass change of the firm column, not the entire ice sheet. Also, clarify “lower” and “higher” mean for changes, e.g. for West Antarctica, does “1.2 Gt/yr higher” mean that the mass change was -3.8 Gt/yr instead of -5 Gt/yr (I made those numbers up to illustrate; in this case higher refers to position on the number line) or higher mean that the mass change was -6.2 Gt/yr instead of 5 Gt/yr (in this case ‘higher’ means greater mass loss)? These numbers are about the entire ice sheet, which is more relevant for altimetry users. Also, the difference in firm mass change is opposite to the change in

total mass loss, and would therefore be confusing to mention. As you can see in Figure 8b,c for the EAIS, the firm height has increased in FDM v1.2A compared to FDM v1.1. Thus less increase in surface elevation change is caused by ice, which has a higher density.

We have specified lower and higher as follows: “When FDM v1.2A is used instead of FDM v1.1p1, the mass change over the period 2003-2015 for East Antarctica is 7.3 Gt/yr lower (187.7 Gt/yr), and for West Antarctica this is 1.2 Gt/yr higher (-243.8 Gt/yr).”

594: Change wording: “forcing or forcing during” reads oddly. We propose to clarify this as follows: “In general, uncertainties in the model formulation, climate forcing or climate forcing during the spin-up cause rather small changes in simulated FAC (<5.2 %).”