General remark

The manuscript that was published in The Cryosphere Discussions covered the period 1960 to 2013. Recently, we have updated the time series to include 2014 as well. In the revised manuscript, we present data up to and including 2014.

Along the way, we discovered two issues with the original data set:

1) The parameterization for the density of fresh snow in the model was not equal to Equation (2) in the TCD manuscript: we used the old parameterization that we used previously for Antarctica (see Ligtenberg et al., TC, 2011, Equation (2). This issue is now fixed. It leads to some changes in the vertical density profiles presented in Figure 3. Moreover, the melt and accumulation fluxes (in mm w.e.) lead to a different height change when the surface snow has a different density. As a result, all surface elevation changes have altered. The integrated effect is shown in the new Figure 11A: ice-sheet wide volume change was estimated at -3900 km^3 in the TCD manuscript. In the revised version, it is -3295 km^3 (-15%).

2) In the forcing data extending to 2013, it was discovered that the snowdrift erosion term for the last few years was set to 0, likely due to a copying error while preparing the firn model input files. It lead to spurious trends in Figure 8J and 8L: locations with mean erosion (negative v_snd) suddenly had a large positive trend towards the end of the time series. On the other hand, locations with mean deposition (positive v_snd) had a strong negative trend towards the end of the time series. This has now been resolved. The new Figures 8J and 8L show more consistent behaviour.

Finally, we decided to express the firn air content relative to the ice density of 917 kg m^{-3}, rather than relative to the pore close-off density of 830 kg m^{-3}. This is more consistent with previous literature on Greenland firn. The effect of this alternative calculation is seen in Figure 6, which now shows firn air content values up to 25 m in the interior, rather than the 15 m in the previous version of the manuscript.

Referee 1 (Robert Arthern)

Equation 1. I think there is a term missing for horizontal velocity divergence within the firn column. Similarly, there is no mention of horizontal advection. How do these compare in magnitude with the other components? Assumptions about these should be stated clearly in the paper.

Below equation 1, we modified the text to reflect these assumptions. We did not choose to include the following back-of-the-envelope calculation: at a flow velocity of 50 m/y (estimate for the lower percolation area), the 11 km horizontal grid resolution equals steps of 220 years of age. Thus, over the model period of 54 years, there is likely a small effect of horizontal advection on firn profiles in the lower percolation area. However, the effect is likely small compared to the current errors due to percolation discussed in the paper.

Equation 1. Sublimation is already included in v_acc, but I think the physical interpretation behind the snowdrift sublimation and snowdrift erosion terms, and how they differ from surface sublimation, needs to be described in more detail. Also, it may be confusing to many readers that v_acc is limited to P-E and does not include snowdrift etc., so it would be worth emphasising that v_acc is not the accumulation rate as it is usually understood.

We have clarified this in the revised text.

P3546. Line 4. v_ice that equals the mean SMB (v_acc + v_snd + v_er + v_me). Shouldn't these be equal magnitude but opposite in sign?

This is correct, and the manuscript has been updated.

Eqns 4 5. Why are the coefficients for Greenland different from those used in Antarctica (e.g. Ligtenberg, 2011)? It would be good to comment on this, and on how much worse the *agreement would have been if the Ligtenberg (2011) values had been used? If there is no physical interpretation for these coefficients and if there is no reason for introducing them other than to improve the fit to the density profiles then this should be pointed out clearly in the paper. The fact that these coefficients are different in Greenland and Antarctica indicates that they are not representing a physical process whereby accumulation rate influences the rate of compaction directly. Instead, they must be correcting for some other process that is missing from the model, but happens to correlate with accumulation rate (albeit differently for each ice sheet). I think this should be pointed out in the paper.*

In the revised manuscript, we acknowledge that the coefficients were altered to obtain a better fit between observed and modelled density profiles. We point out two possible resons for the difference in coefficients over Greenland and Antarctica, but we do not attempt to explain this difference in further detail.

Eqn 6. This equation has the wrong dimensions. It should be divided by the density of ice.

Thank you for pointing this out.

Eqn 6. This equation neglects the presence of liquid water within the firn. This might be a serious limitation under conditions for which a firn aquifer can develop. It would be better to present a more comprehensive treatment that includes liquid water, and then state what assumptions are being made.

We now start the section with the general formula for a mixture of water, ice, and air. We then point out what assumptions we make, and how the equation is simplified as the result of those assumptions.

P. 3550. Line 14. We set v_ice equal to the sum of all other components. Again, should this be the opposite sign from the sum of all other components?

Indeed, and we revised this sentence.

Sections 2.4 and 2.5. These are quite short to be separated as distinct 'methods' sections and could perhaps be combined with the respective discussion sections 3.1 and 3.2.

We acknowledge that sections 2.4 and 2.5 are short compared to 2.1-2.3. Nonetheless, we would like to retain these sections as they are, to keep the separation between models and techniques in section 2, and the results in section 3.

Section 4.4. The present study, with these sensitivity tests included, is just about OK for the time periods under consideration here. However, before doing more long runs using this model I would advise that the use of a reference accumulation rate is replaced by a better representation of the dynamical system representing grain-growth (e.g. Equations B1 and B2 of Appendix B of Arthern et al., 2010).

Within IMAU, we are working on including a dynamical description of the firn system by including grain growth and compaction as a result of overburden pressure rather than by a climatologically mean compaction rate. However, the overburden pressure formulation (in combination with prognostic grain growth) introduces a difficult-to-manage behaviour in the top of the firn column, when the overburden pressure becomes very small. Research on this issue is ongoing.

Figure 1. Needs more tick marks on x-axis. I think it is a log scale, but this is ambiguous.

This is now fixed.

Figure 3 is an excellent summary figure when magnified. In a printed version the text is too small to be readable. I think the figure should be left as is, but care should be given in sizing the figure and checking the proofs so that the text in the figure is legible in the final version. Similar for Fig 5.

These figures have been redesigned, with separate versions for the online (landscape) and the print (portrait orientation) versions.

Referee 2 (Louise Sandberg Sørensen)

General comments and questions:

I find the manuscript interesting, very well written and easy to follow. The results are convincing and presented in a clear way, and the analysis is thorough. The title states '1960-2013' but the presented results cover the period 1980-2013 because the years 1960-80 are used for the model spin-up. Would it not be more appropriate to change the title to 1980-2013?

You are correct that we present results only since 1980. However, the underlying data set, which will be made available, starts in 1960, and the data set can be used to study temporal variability in the period 1960-1979, as long as one keeps in mind that we assume a steady state of the firn layer over that period. We would prefer to keep 1960 in the manuscript title, but if the editor decides otherwise, we will change the title.

You state in the abstract that the model results agree with firn core density data, and in the conclusion you state that you find a very good agreement. While I agree that the model produces convincing results, I think that Figure 3 shows that the model generally predict too high densities – especially considering that the model has been calibrated using the same core densities. I think that this should be pointed out more clearly – already in the abstract.

We have changed the abstract, such that the overestimation of density in the percolation area is emphasized.

I understand the need for the MO correction terms applied to the model results, but it seems that some term(s) are missing in the model since these MO corrections are different from what was found in Antarctica (Ligtenberg et al., 2011). I guess that this could mean that the MO factors could possibly change over time (?) and that this increases the uncertainty when running the model for longer time periods. I think that a more detailed discussion on this should be included in the manuscript.

This point was also raised by Reviewer 1. We have updated the discussion on the choice for MO-parameter values, and we are now explicit about the fact that this is merely done to optimize the fit between the model and observations.

Data from 62 firn cores are used as validation, but only 57 cores are used in Figure 3, 59 in Figure 4. The number of cores used for determining the MO shown in Figure 1 is also not 62. The authors should make it clear why not all of the cores are used.

In figure 1, we used only cores with little surface melt, in order to tune the MO parameters for dry firn compaction only. We included one core with somewhat more surface melt (Das2), because it is the core with the highest accumulation rate, thus expanding the fit range. As a result, we used 22 cores, of which 7 extend to z_830. This is now mentioned in the figure caption.

In figure 3, we use 59 cores, since cores H2-1, H3-1, and H4-1 fall within the same grid box as the cores H1-1 and H5-1 shown in the figure. This is now mentioned in the figure caption.

In figure 4, we used the same 59 cores as in Figure 3. This is added to the figure caption.

Maybe I missed it, but have the authors provided information on the horizontal resolution of the firn model?

The horizontal resolution is mentioned in section 2.2

I think that a reference to Simonsen et al., 2013 would be appropriate in this manuscript, as this paper also describes the work of assessing a firn compaction model in Greenland.

We have added a reference to this work in the introduction, acknowledging previous work.

As also stated by the authors, the use of a mean value of the accumulation rate in Eq (4) and (5) has a significant impact on the compaction rate. I think that the authors describe clearly how and why this represents a limitation, but I think that some more discussion on why this is chosen anyways is needed. I reckon that there are valid explanations for not using the temporally varying accumulation rate, but it is not clear (to me at least) as it is now.

Within IMAU, we are working on including a dynamical description of the firn system by including grain growth and compaction as a result of overburden pressure rather than by a climatologically mean compaction rate. However, the overburden pressure formulation (in combination with prognostic grain growth) introduces a difficult-to-manage behaviour in the top of the firn column, when the overburden pressure becomes very small. Research on this issue is ongoing.

More specific comments and questions:

p. 3543, l. 1-3 : Bottom melt could in principle also be responsible for elevation changes. Or is this term included in what you call 'basal elevation change'?

We have included basal melt as a cause for elevation changes.

p. 3543, l. 3: 'and by the compaction of the overlying firn'. What do you mean with overlying here?

By "overlying" we mean the firn that is situated on top of the ice that makes up the Greenland Ice Sheet. While I am not a native speaker, I hope that the term "overlying" is sufficiently clear for the reader.

p. 3543, l. 15: 'due to firn and SMB' -> 'due to firn and SMB changes' ?

OK.

p. 3544, l. 26-29: Where do you present a time series starting in 1960?

At the end of section 2.3, we motivate why we use only data since 1980 in the figures of the manuscript.

Eq. (1): Do you define vfc ?

We have included a definition for vfc.

p. 3545, l. 11: Here you use capital letters : Greenland Ice Sheet, while most other places you don't. You should be consistent.

We have changed to lower case "ice sheet" throughout the manuscript.

p. 3547, l. 7: Could you also mention the time span in which these cores were collected?

The time span has now been included in the manuscript, in section 2.4.

p. 3548, Eq. (6) : Should be divided by the density of ice.

Corrected.

p. 3551, sect 2.5: It would be nice with a reference here to the figure/table that shows the altimetry elevation changes.

We have added references to the table and the figure about ATM.

p. 3551, l. 26: is this the uncertainty of the derived elevation change? Should the units not be per time then?

These are the uncertainties of the observed elevation itself. We have changed "elevation change" to "elevation" in this sentence.

p. 3552, l.11: 1960-2014? Should it be 1960-2013?

Incidentally, the paper has been updated now to include 2014.

p. 3553, l. 27. You should specify that these are horizontal velocities.

Corrected.

p. 3558, l. 20. Can the authors explain what the rationale for choosing 15% is?

The choice for 15% is motivated in lines 1-18 of section 4.4. It is the best estimate of the uncertainties in the RACMO melt and accumulation fields.

Fig. 1. As mentioned earlier: how many cores used, and why not all?

This is now explained in the figure caption, as indicated above.

Fig. 3. I realize that this is already a very dense figure, but would it not ease the comparison of the model and observation results if they were provided at the same vertical resolution? So this would be the resolution of the cores.

To us, the strength of using the true resolution of the model data is that the figure shows the vertical resolution of the model. We think that the comparison is needlessly complicated if we downsample the vertical density profiles to the core grid. For example, what to do with the depths at which only model data are availble?

Fig. 4. In print it is quite difficult to see the difference in color of the blue dots. Maybe a different color scale would help.

We have chosen a different color scale.

Fig. 5. I would suggest to provide the units for the ATM elevation change map in the figure and not only the figure text. Also increase the text in the map. In the figure text you can change Airborne Topographic Mapper to ATM.

Done.

Fig. 6. The text above the figure is misleading. I would change to either 'Firn air content 01 Sept 2013 (m)' or simply 'Firn air content (m)'.

Changed.

Fig. 11 (b): The colors used for vacc and vfc are almost the same. I would change one of them for an easier interpretation.

Changed.