

## ***Interactive comment on “Elevation change of the Greenland ice sheet due to surface mass balance and firn processes, 1960–2013” by P. Kuipers Munneke et al.***

**L. Sandberg Sørensen (Referee)**

slss@space.dtu.dk

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This study presents elevation (and volume) changes of the Greenland ice sheet obtained from a firn model (IMAU-FDM v1.0) forced by output from the RCM RACMO2.3. The firn model includes compaction, meltwater percolation and refreezing, and is used to estimate the elevation changes caused by SMB and firn processes. The results are suitable for use in altimetry studies and allows for a partitioning of the different contributions to the ice sheet mass changes. The firn model is evaluated against firn cores and airborne altimetry measurements, and it has been calibrated with densities from 62 firn cores from different locations on the ice sheet.

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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 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.

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.

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.

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

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.

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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.

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'?

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

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

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

Eq. (1): Do you define vfc ?

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

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

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

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

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

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

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

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

I think that the figures are informative and of high quality. I do have a few suggestions for corrections though. These are listed below.

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

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.

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.

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.

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)'.

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

References

Ligtenberg, S. R. M., Helsen, M. ~M., & van den Broeke, M. R. (2011). An improved semi-empirical model for the densification of Antarctic firn. *The Cryosphere*, 5, 809–819.

Simonsen, S. B., Stenseng, L., Adalgeirsdóttir, G., Fausto, R. S., Hvidberg, C. ~S., & Lucas-Picher, P. (2013). Assessing a multilayered dynamic firn-compaction model for Greenland with ASIRAS radar measurements. *Journal of Glaciology*, 59(215), 545–

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