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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.

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In this study the authors carry out a simulation of the upper layers of the Greenland ice sheet. This simulation is forced by a regional climate model and includes many of the processes that affect the surface elevation. The model does not include the component of elevation change that is due to the flow of the deeper ice, but it does include the accumulation rate, melting, snowdrift sublimation/erosion, and firn compaction. The study therefore provides all the information that is needed to translate measurements of the change in surface elevation to a change in mass of the ice sheet.

A thorough attempt is made to evaluate the success of the firn model at reproducing the density profile recorded at 62 different sites in Greenland and the temporal changes are also compared to elevation changes measured using laser altimetry. The latter comparison is restricted to regions where the flow of the underlying ice is unlikely to vary enough to cause large elevation changes. The results are analysed to show the contribution from different processes to the elevation change of the ice sheet over different time periods and different parts of the ice sheet.

Overall I found this is a very impressive, useful and illuminating study. These results will provide a very useful dataset that will help enormously in the interpretation of laser and radar altimetry over Greenland. This represents the current state of the art in converting from elevation change to mass. As the authors point out in the introduction this modelling approach represents a great improvement over using a single density to perform this conversion, or trying to partition the density used for the conversion geographically.

The results should not be applied to altimetric time series without some caution, especially in the percolation zone of the ice sheet. Here, this study points to processes that firn models are not capturing well at the moment. In particular, the densities in regions where meltwater percolates into the firn and refreezes are overestimated in the model. A number of possible reasons for this are advanced in the paper. Modelling the compaction of soaked snow and firn still seems to represent a significant challenge.

One weakness of the paper is the formulation of the firn compaction model. Although the model is used in a time-dependent calculation, there are aspects of the model that are fundamentally based upon an assumption of steady state. In simulations of the future, several key parameters in the model are identified using an assumption that the compaction has equilibrated to the climate of the reference period. There is no guarantee that this is true. Here, some sensitivity experiments are performed to evaluate how large the effect of plausible errors in the reference climate might be. This is OK here but in future studies it would be better to take some time to update the firn model and remove the assumptions that require the use of a reference climate. This would not remove all difficulties with model initialisation and spin-up, but would still be worthwhile.

Minor Revisions

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.

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.

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?

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.

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

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ice.

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.

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?

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.

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).

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

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.

Interactive comment on The Cryosphere Discuss., 9, 3541, 2015.