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Interactive comment on “Mass balance of the Greenland ice sheet – a study of ICESat data, surface density and firn compaction modelling” by L. S. Sørensen et al.

Anonymous Referee #2

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General comments

This study considers the remote-sensed elevation change data set over the GrIS as collected by ICESat over the period 2003-2008. It nicely describes some different approaches to get dH/dt values out of this data set, and gives useful insight in the consequences of the different methods in terms of final volume change results and related uncertainty estimates. Then, the calculated ice sheet volume changes are translated into ice mass changes, by taking into account a suite of relevant factors that can impede the translation of the observed dH/dt signal to ice mass changes.

The major strength of this study is the part describing the different methods that can

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be used to determine the dH/dt . This is a welcome contribution to the literature on ice sheet elevation changes, in which it is not always clear which steps are undertaken to reconstruct elevation changes (and their uncertainty estimate) out of the wealth of satellite measurements.

However, some serious issues are associated with the subsequent step to translate elevation changes into ice mass changes. The authors rightfully acknowledge the importance of processes in the firn layer, but the way in which they correct for this seems not correct: several terms in the dH/dt equation are incorrectly neglected; the influence of a seasonal temperature cycle on firn densification is not taken into account; no heat equation is used to calculate the evolution of temperature profiles in the firn column, with consequences for the calculation of the densification rate; the firn densification results are not validated or extensively described; doubtful assumptions are made about the density that is used to convert volume changes to mass changes. I address these issues in more detail below.

Furthermore, I have concerns about the use of the regional climate model data to drive the firn compaction model. The firn compaction results are likely very sensitive to interannual variability in e.g. snow accumulation, surface temperature, melt. Thus, a robust quality-control on this RCM data is crucial. However, the performance of this RCM does not seem to be thoroughly assessed over the GrIS domain, at least any reference to such an assessment is lacking. Since such assessment is beyond the scope of this study, I would advise to either make use of existing and more established SMB reconstructions over this time frame (e.g. Burgess et al., 2010; Ettema et al., 2009), or to decide to perform this analysis on the volume-to-mass conversion in a later stage, and keep the focus on this study on the derivation of dH/dt values. In line with this latter suggestion, I suggest to change the title into one that emphasizes the retrieval of the volume changes, instead of the ice mass changes.

Specific major comments

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Volume - mass conversion

Equation 9 is a correct description of the different terms that influence elevation changes, and in fact is an extended version of the equation by e.g. Zwally and Li (2002), who used the term v_{ice} as a bulk term to include the terms w_{ice} , $u_s dS/dx$, $u_b dB/dx$ and b_m/ρ from equation 9. However, the assumptions made to derive eq 10 are not valid, since in this equation you are suddenly calculating the rate of mass change of the GrIS, and you use the same symbol (b) as used for the surface mass balance. This is where the confusion is introduced:

- if b (in eq 10) is the mass imbalance (db/dt), then it is valid to neglect different terms in eq 9 that can be assumed to be constant over the considered time period (see next points).

- If b is the surface mass balance, then only the terms that are very small over short time periods can be neglected.

- You cannot assume u_s to be constant, lots of evidence exists of rapidly changing ice flow (sliding) velocity over GrIS in the period of observation.

- In line with the previous point: when stating that u_s is constant, you suggest that dynamical ice velocity changes are neglected in this assessment of the GrIS mass balance. In section 7 you do acknowledge that ice velocity changes do play a role in the total mass budget of the GrIS, but the ability of your approach to attribute ice sheet elevation change to changes in ice velocity should be better explained in section 5.

Since you attempt in this paper to explicitly simulate the firn thickness and density changes, it would be much more valuable to distinguish firn volume/mass changes from ice volume/mass changes in the total volume/mass budget of the GrIS, and even make a partitioning of the firn components into a contribution of the SMB anomaly and purely the compaction:

$$dH/dt_{total} = dH/dt_{firn} + dH/dt_{ice} = dH/dt_{SMB} + dH/dt_{compaction} + dH/dt_{ice}$$

$$dM/dt_{total} = dM/dt_{firn} + dM/dt_{ice} = dM/dt_{SMB} + 0 + dM/dt_{ice}$$

In such a way it could become evident that some areas (possibly) suffer from a mass loss without seeing any elevation change: an increase in accumulation (and thus a thickening firn layer) could mask an underlying ice mass loss.

The assumption that all positive dH/dt values above the ELA are due to above-average addition of snow/firn, while all negative values are attributed to ice loss is very debatable, and as such I do not agree with the authors best estimate of the GrIS mass balance. I would say that -166 Gt/yr is the best estimate in this study, but this estimate can easily be modified when a distinction is made between firn mass changes and ice mass changes (see previous point).

firn density modeling

From section 5.1 I deduce that the annual layer thickness determines the vertical resolution of firn layers. Furthermore, you use annual mean values of 2m temperature to drive the firn model. Thus, you cannot solve the heat equation, which means that the temperature distribution in the firn column is not calculated. Hence, seasonal firn density fluctuations are not resolved. This is a major weakness of the analysis, since it was shown by e.g. Zwally and Li (2002) and Helsen et al. (2008) that a detailed reconstruction of firn thickness evolution gives much more insights in the influence of temperature and snowfall variability on the dH/dt observations. Moreover, the densification rate factor (eq. 15) was specifically meant to describe a seasonally varying densification rate, and a mean annual temperature value will not result in the same densification rate as a fully resolved season cycle of the temperature. Furthermore, no clear reason is given that justifies this treatment, while I suppose you have all the necessary data available from the RCM to perform a more detailed firn compaction simulation.

No results are presented considering the density profiles resulting from the use of the firn density model forced by the RCM data. Since GrIS mass change results are strongly affected by this firn correction, some evidence of the validity of the firn com-

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paction results would be welcome.

Related to this, I suspect that using a single value for beta ($\beta=8$) over the entire GrIS will lead to a large overestimation of the snow compaction dependence of temperature in warmer areas than Summit, since $\beta=8$ is a best value of the parameter fitted to match only the data at Summit. You need to use a temperature-dependent parameterization of beta, e.g. the one from Li et al (2003), and then using a limit on the lower value of beta.

The approach of estimating the firn compaction velocity from the upper 15 annual layers may lead to errors. It is implicitly assumed that each 16th annual layer (and every other deeper layer) does not contribute to the firn compaction velocity. This assumption is not valid. Maybe its effect is best illustrated with a simple example: suppose an anomalous thick annual layer formed in 1989, this layer leads to a larger-than-average firn thickness for 2003, while in the next year the rejection of this layer leads to a decrease in calculated firn thickness, and also to a large decrease in air space. It is then concluded that the firn compaction velocity decreases, while in reality the firn compaction is still larger-than-average if all other years receive an average amount of snowfall.

I agree with the authors claim that the HIRHAM5 climatology is too short to derive a robust reference firn density profile, but at least the effect of the above-mentioned assumptions needs a more thorough assessment. Related to this: the error in the firn compaction velocity (Figure 4f) is thus much larger than only the error in the fit seen in Figure 4c.

The density of the upper layer is taken for the translation of dH/dt over the observed period to ice mass changes, why? It would be better to take the mean density of the layers accumulated in the period of observation, and you have this information available. Also for the calculation of the error in the total mass balance (section 7), no contribution of an uncertainty in firn density is taken into account. This leads to an

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underestimation of the total uncertainty given in Table 2.

Regional Climate model

Using data from a Regional Climate Model as input for your densification model requires a detailed quality check. You do not give any reference of an assessment of this RCM, is there any? If not, a much more detailed description of the RCM data is needed to warrant the use of this data set.

Why do you need to find the ELA from a parameterization by Box et al. (2004), don't you have this information readily available in the HIRHAM5 RCM?

The interpolation technique (section 5.3) that is used to produce the 5x5km data clearly suffers from problems, considering the spurious signal off the east coast in the 3rd panel of Figure 3. This needs to be improved.

If your RCM data is better constrained, then this study offers the possibility for a better partitioning of the GrIS volume changes into different components. In line with the approach of Prichard et al. (2009), who use the same ICESat data, the dynamical thinning could be isolated from the SMB-related signal in dH/dt . Your RCM data offers the opportunity to directly quantify the SMB-component of the dH/dt data, and as such attribute the remaining elevation change to ice velocity variations.

Minor comments

p. 2105, line 14 and p2106, line 15: these lines (“surface density modeling”) suggest that surface firn processes can explain all volume change.

p. 2108, last line and p. 2109 first line: here you need to mention that a seasonal cycle in dH/dt can also be introduced by the temperature-dependent firn compaction.

p 2109: While using different methods to solve the dH/dt data, a seasonal amplitude D is taken into account. The magnitude of D is never mentioned. Can this amplitude be explained in terms of seasonality in snowfall from the RCM? Can distinct spatial pat-

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terns in this amplitude be recognized? Are these the result of temperature-dependent densification as Zwally & Li (2002) showed?

p. 2112, line 13: the criterion for a valid crossover location is that the two closest points are not greater than 500 m. Why are so many cross over locations locations rejected based on this criterion, since the along-track footprint distance is only 170 m?

p. 2115, line 24-25: are these larger variances of method M1 related to the low resolution of the DEM?

p. 2115: What is the reason for the skewness in the results in Figure 2.

p. 2117, line 8-9: it is not realistic to expect that that the steady state reference firn column (based on a constant temperature) is a good approximation of the steady-state of a time-dependent (and thus using a variable temperature) firn density model.

p. 2117: define t_2 in eq 11.

p. 2118, lines 4-8: Note a recently published work of Arthern et al. (2010) that improved the densification rate expressions based on field measurements of firn thickness changes.

p. 2118, line 23: why not use the skin temperature of the surface snow? This would be a better boundary condition for the firn model. Section 5.4: the assumption that 60 % of the run-off (as given by the RCM) is refrozen as ice lenses in the snow pack is a very crude estimate, and this treatment of the refreezing is not even always physically possible: a location just above the ELA probably does not have enough pore space in the annual firn layer that is needed for the refreezing of the melt water, i.e. those annual layer will be fully saturated with water.

Figure 6: Unit of mass change cannot be in Mt/yr in a distributed plot, since this suggests a dependence on grid size. Better use m ice equivalent yr⁻¹.

References

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