Reply to Anonymous referee #2

We thank the Anonymous referee #2 for sharing his/hers insight in the complicated processes of firn compaction and how to model it. We agree with the comments and address them below.

General comments

1. Paragraph 3 (p. C1150)

The referee is critical towards our methods of converting volume change to mass change. We would like to emphasize that ice dynamics is considered by our analysis, but we can see how the text may be misleading. Basically the concept is the same as in the newly published article by Zwally et al. 2011. As suggested by the referee we have now included the seasonal temperature cycle in modelling the firn densification and use the heat equation to calculate the evolution of temperature profile. We believe this addresses the issues satisfactorily.

The main differences between the approach in the paper by Zwally et al. 2011 and our approach are:

(1) Inter-comparison between different sampling and interpolation methods of ICESat data, and firn modelling with higher spatial resolution.

(2) We use the HIRHAM5 RCM to provide temperature and accumulation history over the ERA-INTERIM period (1989-2008). The HIRHAM5 RCM data cover the observation period of ICESat, and contain the spatial and temporal resolution over GrIS in high resolution.

(3) We include ice lenses in our firn density model.

(4) We model the combined effect of accumulation and temperature on the firn column, instead of separating the two as done by Zwally et al. 2011.

We believe that our results provide an alternative and novel estimate of the mass balance of the GrIS based on ICESat data. Depending on the evaluation of the ICESat data, we generally find a similar or larger mass loss than Zwally et al. 2011, showing the impact of interpolation sampling and firn density as pointed out in the short comment by J. Bamber.

2. Paragraph 4 (p. C1150)

The RCM model used here is an upgraded version of the HIRHAM model that has been used in several studies of the accumulation and climate over Greenland. This model has been validated both with ice core data and automatic weather station data and is shown to perform well over Greenland. (Dethloff et al. 2002, Kiilsholm et al., 2003, Box and Rinke, 2003, Stendel et al 2008) The upgraded version of the model, HIRHAM5 has been applied to downscale the ERA-Interim reanalysis data set from ECMWF at a number of resolutions (0.5, 0.25 and 0.05 degrees). The highest resolution (0.05 deg. or 5.5 km) model run is used here and this model run is validated in the master thesis work by Maria E. Wulff (University of Copenhagen) and in a publication where the model runs of different resolutions are validated against DMI met office data, GC-Net weather station data, as well as ice core data from the ice sheet for accumulation validation (P. Lucas-Picher 2011, in preparation). The validation study shows that the model performs well in this area.

Specific major comments #1: Volume – mass conservation.

We see how the text can be misleading, and after replying to the different parts of the specific major comment #1, we propose a clarified text to replace section 5 in the manuscript.

1. Paragraph 1 (p. C1151)

As mentioned by the referee equation 9 is an extended version of the version in Zwally and Li 2002, or the full equation for balance velocity by Paterson 2002. However the part leading to equation 10 should be explained better.

The dynamic part is not strictly neglected as it may appear when rereading the lines 18-24 page 2116. Since the dynamic part is re-appearing in equation 16 (the assumed density of mass loss).

Let us elaborate on the thoughts behind, the derivation of equation 10 which have to be clarified in the manuscript. Below is the revised version of equation 10. The goal of this study is to derive the mass change of the GrIS, and the physics behind this may be boiled down to a few processes: (mass gain 1) Addition of mass from precipitation, (mass gain 2) refrozen water at the base of the ice sheet, (mass loss 1) calving, (mass loss 2) surface and basal melt not refreezing.

The W_c and W_{br} terms in equation 9 are the only two terms, which are independent of a mass change of the ice sheet and therefore have to be subtracted prior to a volume to mass conversion.

The ice sheet dynamics $\left(W_{ice} + u_s \frac{dS}{dx} - u_b \frac{dB}{dx} \right)$ will be observed (from ICESat) as an elevation change, and when the proper density is related to the observed elevation change, the dynamics are in fact included in the study. The formulation of equation 10 was chosen, to summarize the physical considerations of a mass to volume conversion, however the equation is misleading and should revised to:

$$\frac{dM}{dt} = \frac{dH_{corrected}^{ICESat}}{dt} \rho^*$$

where M total mass and ρ^* is the density described in section 5.5. However to clarify the section the density considerations from Section 5.5 should be moved to 5.

2. Paragraph 2 (p. C1151)

The referee suggests partitioning SMB anomalies and the pure firn compaction, as done in the new article by Zwally et al. 2011. However, the way we are accounting for the firn compaction is by deriving the changes in air volume, illustrated by the top panel of figure 4. In combination with the density considerations, this is the same result as partitioning SMB anomalies and the pure firn compaction.

3. Paragraph 3 (p. C1152)

We use different densities above the ELA depending on whether the elevation is increasing or decreasing. The physical processes involved justify this. When an elevation increase is observed above the ELA this is due to an addition of snow or ice dynamic (inflow of ice). We assume that this is due to addition of snow, and we use the density of surface firn/snow to calculate the mass change. We estimate the error involved in this assumption in section 5.5 (see comment below). Above the ELA, for a decreasing surface elevation, we assume that this is due to ice dynamics and corresponds to removal of ice, and we use the density of ice to calculate the mass change.

We then suggest replacing line 18 (p. 2116) to line 2 (p. 2117) with:

In the context of converting observed volume change to mass change the two non-mass related terms $\left(W_c \text{ and } \mathrm{W}_{br}\right)$ *in equation 9 have to be subtracted from the observations before estimating the mass change*

$$\frac{dM}{dt} = \frac{dH^{ICESat}_{corrected}}{dt}\rho *.$$

To account for the dynamic part of equation 9 $\left(\mathrm{W}_{\mathrm{ice}} + \mathrm{u}_s \frac{dS}{dx} - u_b \frac{dB}{dx}\right)$ *the density* $\rho *$ *is varying depending on the physical processes behind elevation change above or below the ELA and is assumed to be either the density of ice or firn, depending on the location on the ice sheet.*

In the ablation zone, defined here as the area below the ELA, all elevation changes are assumed to be caused by ice. Above the ELA, in the accumulation zone, an elevation increase is assumed to be caused by an addition of snow/firn. However, an elevation decrease is assumed to be caused by ice dynamics and therefore corresponds to removal of ice. The surface density is then paramterized by

$$\rho * = \begin{cases} \rho_s \text{ if } \dfrac{dH^{ICESat}_{corrected}}{dt} \geq 0 \text{and } \mathrm{H} \geq \mathrm{ELA} \\ \rho_i \text{ else} \end{cases}$$

where ρ_s *is the surface density of firn including ice lenses, and is given by*

$$\rho_s = \frac{\rho_0}{1 - \frac{r}{b}\left(1 - \frac{\rho_0}{\rho_i}\right)}$$

Here, r is the amount of refrozen melt water inside an annual firn layer, $\rho_i = 917\frac{kg}{m^3}$ *and* $\rho_0 = 625 + 18.7\mathrm{T} + 0.293\mathrm{T}^2$ *is the temperature dependent density of new firn before formation of ice lenses (Reeh et al., 2005).“*

This revision to the manuscript will remove the first part of section 5.5.

Specific major comments #2: Firn density modeling.

1. Paragraph 1 (p. C1152)

The RCM provides all information needed to make a detailed study of the firn compaction in sub-annual resolution. As pointed out by the referee sub-annual resolution should be included and we have now modelled the monthly layering of the firn above the ELA. And this will be included in a revised mauscript.

To model in sub-annual resolution the seasonal temperature profile is needed and have been added to the model. This addition made us revise the parameterization of the c_0 and c_1 constants to follow the newer model published by Arthern et al. 2010, as suggested by the referee.

We propose to change the text in page 2118 line 4-21(From *"The Zwally and Li…."*) to the following :

"Following Arthern et al. 2010 the densification constant is given by the Nabarro-Herring type creep

$$c = \begin{cases} 0.07 b(t) g \exp\left(-\frac{E_c}{RT} + \frac{E_g}{RT_{av}}\right) & \text{for } \rho < \rho_c \\ 0.03 b(t) g \exp\left(-\frac{E_c}{RT} + \frac{E_g}{RT_{av}}\right) & \text{for } \rho_c < \rho \end{cases}$$

where g is the gravity constant, E_c and E_g is the activation energy (60 kJmol^{-1} and 42.4 kJmol^{-1} respectively). T_{av} is the average temperature and T is the temperature at a given depth in the firn derived by surface temperature fluctuations from the RCM and the normal heat equation,

$$\rho c \frac{\partial T}{\partial t} = K \nabla^2 T - \rho c \left(u \frac{\partial T}{\partial x} + v \frac{\partial T}{\partial y}\right) + \left(\frac{\mathrm{d}K}{\mathrm{d}z} - \rho c w\right) \frac{\partial T}{\partial z} + f \quad .$$

Where c is the specific heat capacity, K is the thermal conductivity and f is internal heat production [Paterson 2002]. The heat equation is solved following Schwander et al. 1997 using a Crank-Nicolson scheme."

As suggested both in the short comment by J. Bamber and this present review, the HIRHAM5 RCM should be used to determine the ELA, instead of the parameterization by Box et al. 2004. The revised firn compaction model, the new ELA calculation and changing ρ_i from 900 to 917 kg m^{-3} is resulting in a revised mass balance estimate for the GrIS of

Revised results	ICESat 1a	ICESat 1b	ICESat 2a	ICESat 2b	ICESat 3a	ICESat 3b
With firn comp.	-226	-168	-181	-135	-238	-177

Without firn comp.	-262	-187	-217	-153	-274	-196

Old result from TCD	ICESat 1a	ICESat 1b	ICESat 2a	ICESat 2b	ICESat 3a	ICESat 3b
With firn comp.	-199	-157	-155	-121	-210	-166
Without firn comp.	-256	-190	-212	-154	-267	-199

Difference (New- old)	ICESat 1a	ICESat 1b	ICESat 2a	ICESat 2b	ICESat 3a	ICESat 3b
With firn comp.	-27	-11	-26	-14	-28	-11
Without firn comp.	-6	3	-5	1	-7	3

Where a is with remote removal of ice (see manuscript) and b is without.

These results are based on our current ICESat derived elevation changes and will be slightly different, when the methods for deriving elevation change have been revised according to suggestions by anonymous referee #1

2. Paragraph 2 (p. C1152-C1152)

With the extensive firn modelling work, which forms the basis for the firn compaction modelling, a validation study of the model is interesting. However, this is outside the scope of the presented manuscript.

3. Paragraph 3 (p. C1153)

We now use the parameterization by Arthern et al. 2010, which eliminates the need for an enhancement factor beta.

4. Paragraph 4 (p. C1153) and Paragraph 5 (p. C1153)

The maximum number of years is determined by the start of the ERA-Interim reanalysis period. Therefore, the modeled firn column at the beginning of 2003 is consisting of 169 monthly layers. This number of layers is then the basis for the modeled change in top firn air content. As the referee is commenting this might lead to a larger-than-average firn compaction correct. We propose to add the following to page 2122 line 24.

"Only comparing the thickness of the top 169 monthly layers, may lead to an overestimation of the firn compaction velocity and need to be accounted for in the error estimate of the firn compaction."

Therefore, as it is stated on page 2123 the error of the firn compaction is difficult to estimate, and we propose to add the following sentence at the end of line 17 (p. 2123)

"With the possible overestimation of the firn compaction, as stated earlier, the higher error estimate is properly the most realistic"

5. Paragraph 6 (p. C1153)

We have chosen to use a mean of the surface density over the period. This choice is also related to physics behind the addition of mass to the ice sheet, which only can come in the form of snow precipitated on the surface. As stated in the reply to J. Bamber the maximum error of the volume to mass conversion is 38 Gt/yr.

Specific major comments #3: Regional Climate Model.

1. Paragraph 1

References from general comment paragraph 2 will be added to section 5.2.

2. Paragraph 2

The ELA has for simplification been estimated from the Box parameterization, and as explained above we are now using the HIRHAM5 RCM to estimate the ELA.

3. Paragraph 3

The figure shows the HIRHAM RCM output (middle panel) (0.05x0.05 deg) and the interpolation to the equal distance grid (5x5km) (right panel). The signal off the coast of east Greenland is not related to the interpolation, since it is seen in both the middle and right panel of the figure. It is attributed to the fine resolution of the coastal topography, giving the possibility to model the tunneling effect of major outlet glaciers.

Minor comments:

1. p. 2105 l. 14

"Subsequently, the corrected volume change is converted into mass change by surface density modelling." Will be rephrased to:
"Subsequently, the corrected volume change is converted into mass change by the application of a simple surface density model."

2. p. 2106 l. 15

"The conversion of the derived dH/dt values to mass changes is based on firn compaction and surface density modelling, forced by climate parameters from a regional climate model (RCM)." Will be rephrased to:
"The conversion of the derived dH/dt values to mass changes is based various correction terms and the simple surface density model, where the firn correction and the surface density models are forced by climate parameters from a regional climate model (RCM)."

3. p. 2108

Will be rephrased to:
"An observed elevation difference between tracks will also include a seasonal signal, caused by variations in accumulation, flow, melt and temperature dependent firn compaction rate.

4. p. 2109

Spatial patterns in the amplitude of the seasonal signal can be recognized. We propose to add a figure showing the amplitude in the supplementary material of a revised manuscript.

5. p. 2112

We believe it to be due to lack of data along some tracks.

6. p. 2115 l. 24-25

As also pointed out by anonymous referee #1, the low resolution of the DEM leads to a larger part of the topography being unresolved. This again will lead to a poorer fit of the data and larger variances.

7. p. 2115 (Figure 2)

We do not expect the distributions of the volume change estimate to be completely symmetric. This would only happen if data were normal distributed and all steps in the data processing were linear. The confidence intervals shown with red bars are related to the distribution based on the 1000 bootstrapped volume estimates. The point estimates, shown with red dots are based on the original dH/dt values and will therefore not necessarily be centered.

8. p. 2117 l. 8-9

We agree that the steady-state reference firn column is not a good approximation to the steady-state time dependent firn density model, which is the reason for not deriving the firn compaction velocity directly from eq. 11 but from the 169 monthly layers.

9. p. 2117 eq. 11

The first sum over t2 is the addition of a surface layer, and and we propose to rephrase "where *t*0 is the time of deposition, …" to "where *t*0 is the time of deposition, t2 is the addition of a new surface layer, …"

10. p. 2118 l.4-8

As mentioned above we will change the firn modelling according to Arthern et al. 2010.

11. p. 2118 .23

In the new model run the temperature is changed to the surface temperature.

Regarding the 60% assumption, see the reply to the short comment.

12. Figure 6

The figure will be changed to show m_{ice} equivalent per year.

References

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