

## Response to reviews of “Time-evolving mass loss of the Greenland ice sheet from satellite altimetry”, paper tc-2014-6.

For readability, the reviews are cited in full in this document. Our response is presented in italic font, where actual adjustments to the manuscript are in bold. We thank both referees for their careful evaluation of the submission and their comments, which were appreciated and which have led to a number of improvements in the paper.

### Review 1 (by anonymous referee #1)

p. 1058, line 4: Please state that the “ice-sheet-wide” mass loss from GRACE agrees well with the IOM method. GRACE cannot deliver mass loss of individual glaciers.

*Changed as suggested: “...reconstructing ice-sheet wide mass changes....” (line 3).*

p. 1059, line 6-7: This paper was submitted before Khan et al. (2014), which actually do provide shorter term mass loss estimates from altimetry (3 year interval). Please include Khan et al (2014), also in table 3. Khan et al (2014) provides mass loss estimates for 2003-2006, 2006-2009.

*Thank you for pointing us to this paper, which is indeed useful. We added the Khan et al., (2014) results to Table 3 and to the discussion: “The near doubling in mass loss that Khan et al., (2014) find based on ICESat, i.e., 172 Gton yr<sup>-1</sup> for 2003--2006 to 292 Gton yr<sup>-1</sup> for 2006--2009, is a larger increase and larger absolute value than we obtain for the latter period. We note, however, that their GRACE-based estimate of 257 Gton yr<sup>-1</sup> for 2006-09 is identical to our value for this period.. “*

p. 1064, line 5-10: Jakobshavn Isbræ (also other glaciers) has velocities of more than 10 km/yr. Have you removed the points near the glacier front in figure 2a? If so, please state it in the text.

*We capped the axis of the figure for readability. The points near the glacier front (of which there are very few) have indeed very high velocities. They are included in the relationship but not shown in Figure 2a. We also mention this now in the caption of Figure 2a: “Note that the x-axis is limited at 3 km/year for clarity. The few points that have higher velocities are included in the relationship but not in the plot.”*

p. 1068, line 13-20: Figure 4 show trends for 2003-2009. Please show trends for 1995-2001. Figure 4b shows dH/dt caused by firn compaction. What is the total rate in km<sup>3</sup>/yr for the GrIS? List the rate in the text, as it will make it easier to compare with other studies.

*We added a figure showing modelled trends for 1995-2002. For 2003-2008 we calculated the requested value, which amounts to 20 km<sup>3</sup>/yr (on average about 1 cm/yr). However, the anomaly in firn compaction, which determines this value and is shown in Figure 4 (and now also 5), strongly depends on the previous anomalies in SMB: a thicker than usual snowpack will cause more than usual firn compaction (i.e. an downward trend). Therefore, we do not think this number of 20 km<sup>3</sup>/yr is easily comparable to other studies and we choose not to add it to the text.*

p. 1069, line 8: I assume the elastic uplift of bedrock has been taken into account in the final mass loss estimate? If so, please mention it in the text. GIA is small, less than 2 Gt/yr and can be ignored (ice5g).

*We did not include elastic uplift as a correction to the volume change estimate as its impact is significantly less than that of other uncertainties in our calculations.*

p. 1073, line 24: I do not like that you state that mass loss peaked around 2006. This is true only if you ignore 2010-2014 data. As many GRACE studies have shown, 2010 and 2012 were extreme years with huge melt and mass loss.

*We changed the formulation of the sentence and extended the sentence: **Mass loss increases after 2000 until about 2006, and then decreases slightly between 2006 and 2009. This is broadly consistent with gravity-derived mass trends which show a reduction in mass loss in 2007 (Rignot et al., 2011). Other studies (e.g. Khan et al., 2014) have shown that after 2010 greater ice-loss has been recorded.***

p. 1074, line 10-13: I am not sure whether delay can explain discrepancy for Jakobshavn (see figure 8) between this study and Howat et al. (2011). However, it should be mentioned that independent GPS measurements of crustal uplift (Khan et al., 2010) support a mass loss rate of  $\sim 20$  Gt/yr during 2006-2009 (as suggested by this study) rather than  $>30$  Gt/yr (as suggested by Howat et al., 2011).

*Thank you, we added this to the manuscript in the discussion of Figure 8 ("**Besides, as a third estimate for Jakobshavn (Khan et al., 2010) gave a mass loss of about 20 Gton y<sup>-1</sup> between 2006 and 2009, which is comparable to our results.**")*

p. 1074, line 17: What are the main differences between RACMO and RACMO2?

*There are many differences between RACMO1 and RACMO2, and RACMO 2.1 (the version used in this paper) and RACMO 2.0, including the atmospheric physics and the snow model. The difference most relevant for this study is the introduction of a physical snow model which includes effects of snow/firn properties on surface albedo. The properties of the current version of RACMO(2.1) and the snow model are extensively described in Ettema et al., (2009) and references therein.*

Figure 8. There is something wrong with the way you compare mass loss from this study with mass loss from Howat et al. (2011). According to your figure 8, Helheim, Kanger and Jakobshavn were in mass balance in 2000 ( $dM/dt=0$ ). This is very unlikely. I suggest you remove the first point from the time series. Simply start from 2001.

*This is indeed an artefact of the way we plotted the time series. We removed the points at 2000 from the plot as suggested.*

Review 2 (by anonymous referee #2):

Major points: P1063: It is not entirely intuitive as to why velocity has been selected as a predictor of  $dh/dt$ . Velocity divergence and/or elevation would make more sense to me. Rapid flow in itself does not necessitate a change in elevation. Looking at Figure 9 of Hurkmans et al. JGR 2012 I see little improvement in prediction skill between ordinary kriging (OK) and space-time kriging with external drift (SP-KED) when applied to the dynamically active Jakobshavn Isbrae. I suspect that using OK over the entire ice sheet would give nearly identical results as presented here. My feeling, after reading both papers, is that SP-KED using velocity fields is a novel approach but does not add value to region-wide estimates of volume change. At a minimum, it would be nice if the authors could show the impact of using OK or SP-KED when applied to the entire ice sheet as SP-KED requires the ingesting of ancillary velocity data whereas OK does not. I see that this is included in Figure 7 but specific numbers in Gt/yr would also be useful. Additional evaluation of interpolated results against ATM data is also warranted for all of GrIS.

*In Hurkmans et al., (2012) the justification for using velocity is described extensively. Velocity divergence would make sense according to the continuity equation, but the area in which spatial gradients in flux divergence are large enough to yield trends in  $dH/dt$  are very small. A relation between elevation and  $dH/dt$  is sometimes used for interpolation (hypsometric approach), but there is no clear relationship between elevation and dynamically induced  $dH/dt$ . For KED, the point is that the spatial gradient of velocity is related to the spatial gradient of  $dH/dt$ . The former is extracted from the velocity map by the algorithm. It should be noted that this relationship is not fixed but estimated separately for every point and time step. Multiple trends  $dH/dt$  can result from the same trend in velocity, as constrained by surrounding altimetry measurements.*

As to Figure 9 of Hurkmans et al., (2012), it is not entirely clear to us what you mean with “prediction skill”. The figure shows that (ST-)OK and (ST-)KED perform nearly the same in the cross-validation exercise (which is expected as there are only very few data points in the dynamically active area). KED does reduce both the bias and the spread of the difference with ATM considerably (e.g. see 2005 and 2006 in Figure 9b).

*It is certainly true that the added value of KED is much greater over (relatively small) dynamically active glaciers and diminishes somewhat at larger scales. In fact, in the current paper much of the ice sheet was interpolated using (ST-)OK to prevent artefacts in the interior that are caused by noise in the velocity (where values are small). The total amount of mass lost over all the outlet glaciers is maybe relatively small when the whole of Greenland is considered but not negligible as can be seen in Figure 7. The difference between ST-OK and ST-KED is variable between years but is on average for the entire period 21 Gton/yr. We added this to the text (beginning of Section 6), where we already stated that the relative difference varies between 10 and 20%.*

Section 4: Firn density modeling. If volume change is going to be converted to mass change, some attempt needs to be made to validate the modeled firn compaction results.

*The model that we used is not new but is published and has been validated with observed depth-density profiles at ice-core sites. Additional validation in this study would require additional data which we did not have. Even then, measurements would only be available at a few points. Instead, we ran the model multiple times with perturbed input to produce a (as realistic as possible) error estimate, which is then incorporated in the final uncertainty band. This goes somewhat beyond what previous studies (e.g. Sorenson et al 2011) have done in terms of firn compaction uncertainties.*

P1069, L19 I am concerned by the authors comment that “Discrepancies between the modelled SMB and Halt, in particular underestimation of accumulation anomalies or overestimation of melt anomalies by the model, lead to positive  $dH/dt$  values that are then associated with ice dynamics and assigned an ice density. Especially in the ice sheet interior, this is clearly not realistic.” If this is that case, why use the model at all?

*Because there is no other way of including spatially distributed estimates of SMB and firn compaction. Neither model nor observations are perfect, so it is not strange that there are some discrepancies. We can explain them and correct for them in this way.*

All of section 5 needs to be revisited. I found it nearly impossible to follow what should be a simple volume to mass conversion:

1. Calculate change in volume,
2. Estimate mass change assuming constant firn density profile (use a density of  $\sim 900$  kg/m<sup>3</sup>),
3. Correct for changes in firn pore space using model results.

*We agree that the structure of paragraph 5 may be confusing. Although the conversion of volume to mass is essentially simple, most of the paragraph deals with the error estimates, which is much more complicated. Especially the spatial aggregation of errors needs explanation. We reorganized the paragraph and think it is now more logical. In addition, this point is related to the previous one. In the interior the elevation changes are relatively small. Corrections using model results will have a relatively large impact and because neither model nor observations are perfect this adds uncertainty, complicating the described procedure.*

Figure 7: What caused the mass gain in 2001 and 2002 if not SMB?

*The increase in 2001 and 2002 is only visible in OK and KED results (i.e. no space-time kriging) and is caused by the fact that this is the transition period between ERS-2 and ICESAT, meaning that neither has a full 3-year period and estimates with low enough standard errors to be included. Space-time kriging includes more data and does not include this artefact. This is explained on page 1073 (around line 10).*

Minor Comments:

P1062: Would it be valuable to remove (as best possible) the seasonal signal from ERS-2 elevations? Otherwise there is a potential for trends to be biased by seasonal sampling.

*The ERS-2 time series all encompassed complete years. In the 3-year linear trends that we extracted from them, therefore, seasonal effects averaged out. In principle, this would reduce the noise in the annual trends. However, we found considerable variability in the magnitude of the seasonal cycle, making it difficult to remove it without creating artefacts.*

P1063: "We thus assume that by using elevation change rates, the differences between the datasets largely cancel out." I wasn't able to totally follow the authors logic. Can the authors please expand? Obviously the authors are not referring to differences in the spatial and temporal sampling characteristic of the sensors.

*We are referring to any structural differences (biases) that might (and do) exist between ERS-2 and ICESat in absolute elevation measurements. By only comparing  $dH/dt$  estimates these errors will to a large extent average out, assuming that they are relatively constant in time. We reformulated this sentence: "**We thus assume that by using elevation change rates, biases in estimates of absolute elevation between the datasets cancel out.**"*

P1064: "The slope of the linear regression is, thus, an easily obtained indicator for whether or not a glacier is thinning dynamically." Is this true in all cases? I would expect lower surface velocities in the accumulation zone and higher surface velocities near the glacier terminus, regardless of dynamic thinning. In the absence of dynamic thinning, I would expect a relationship between SMB,  $dH/dt$  and elevation if the glacier was out of equilibrium with the atmosphere. I would expect that this would generate correlation (positive or negative) between surface velocity and  $dH/dt$  that would be unrelated to flow divergence.

*We agree that a relation is possible between velocity and SMB in the way you suggest. This is visible in Figure 2a: glaciers without much dynamic thinning still have a range in  $dH/dt$  of about 2 m/yr, with slightly lower (higher in absolute numbers)  $dH/dt$  at higher velocities. Generally, however, these slopes are much less steep than glaciers that are known to be thinning dynamically, because the amount of thinning is much larger. As can be seen in Figure 2A, this results in two distinct clusters of points.*

P1064 Figure 2c is not labeled

*We do not see any missing labels here.*

P1064 The pearson correlation coefficient is defined as rho and R (see Figure 2c).

*We now use 'r' for the Pearson correlation coefficient (see below).*

P1066, L19: change "SMB = SIR >= 0" to "SMB == SIR"

*We changed it into "**SMB == SIR >= 0**"; to emphasize the contrast with the ablation zone (with negative SMB), we think it is better to explicitly state that in the SIR zone, both are positive.*

P1069, equation 5: Simply use "A" in place of "AxD"

*Throughout this section, we replaced "AD" by "A". Below Eq. 5, we now state that a correction factor for area distortion (Eq. 6) is taken into account in A.*

P1069. L9: You already used rho for the correlation coefficient. I would suggest using "r" for the correlation coefficient

*We agree with this and now use "r" for the correlation coefficient throughout the paper.*

P1074: I am concerned by the statement "If our results for Jakobshavn Isbræ are shifted back by about 2 yr the mass trends agree well in magnitude with Howat et al. (2011)." The authors need to dig deeper to determine the reason for the disagreement. It may simply be that their interpretation methodology needs further refinement.

*Our results do disagree with Howat's, but we agree that a shift can only partially be explained by the delay due to radar altimeter sampling. Reviewer 1 pointed us to other, independent estimates of mass loss for Jakobshavn (Khan et al., 2010), which are more consistent with our results. Therefore the Howat et al., (2011) result might be an overestimation of mass loss. We removed the here cited statement, and added a reference to Khan et al., (2010). See also our response to reviewer 1.*

P1075: The following needs further justification: "For the period 1992–1994 we assume the ice sheet is close to balance and we extend our time series for these years with 0\_50 Gtyr<sup>-1</sup>"

*We have no way of knowing what our estimates would be before 1995, but we do know that immediately after 1995 the mass balance fluctuates around 0 with a considerable error margin (about 50 Gton/yr). To meaningfully compare our cumulative numbers with those of sources that did take into account 1992-1994, we have to make an assumption, which is to extent our results for roughly 1995-1997 backwards in time. We added: "...**which seems reasonable considering the period 1995-1997 in Figure 7,**..." to the above statement.*