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Interactive comment on "Time-evolving mass loss of the Greenland ice sheet from satellite altimetry" *by* R. T. W. L. Hurkmans et al.

Anonymous Referee #2

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Summary: This paper estimates mass changes of the Greenland Ice Sheet (GrIS) between 1995 and 2009 from interpolation of ERS-2 and ICESat altimetry data. The authors employ a novel interpolation approach that relies on the relationship between glacier dh/dt and surface velocity. Elevation changes are converted to mass changes accounting for evolving firn density using an empirical model. The paper is thorough and a welcomed contribution.

Review: Overall the authors have made a commendable effort to synthesis these two altimetry datasets. Below are my suggestions for further improvement:

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 where as 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.

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.

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?

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 ∼900 kg/m3), 3. Correct for changes in firn pore space using model results.

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

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.

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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.

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.

P1064 Figure 2c is not labeled

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

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

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

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

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.

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-1"

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