These comments are not a formal review of the paper but are designed to help improve the paper and seek clarification on some points that are unclear in the manuscript.

General comments

This is a valuable and timely study that looks at, specifically, the impact of interpolation and sampling on converting dh/dt measurements from satellite altimetry (this study could equally apply to radar or laser altimetry) to a volume change and subsequently a mass change. The focus, and the most insightful and rigorous part of the study is the former: the conversion from dh/dt to dv/dt. The second component, from dv/dt to dm/dt has a number of issues and limitations which make it less general and which, in my opinion mean that it premature and misleading to provide an estimate of mass balance in this study.

This does not detract from the value of the work but the focus and **new** information and understanding is not on mass trends but on how sampling affects a mass trend that might be derived from altimetry. In my view, therefore, the title of the paper is misleading and should be changed to something like "The impact of interpolation, sampling and firn density on volume change estimates from satellite altimetry". I will explain my reasoning below.

Specific comments

Equation 10 cannot be correct because it states that the elevation rate measured by ICESat is a function of SMB only. This is clearly incorrect, as shown by eqn 9. Because U_s ds/dx and Ub dB/dx are assumed (incorrectly) to be constants it does not mean they are zero. This issue affects much of the rest of the analysis, which assume that volume changes are due to SMB and firn effects. During the measurement interval (2003-2008) we know that du/dt $\neq$ 0. Equation 10 is only approximately valid in the ice sheet interior where both flow divergence and dh/dt are small.

5.1 Firn compaction.

It has been known for a long time that firn compaction is an important [*Arthern and Wingham*, 1998], and in places, dominant component of the elevation trend [*Reeh*, 2008]. In fact in the paper by Reeh he discusses how the mass driven elevation rate may only be 10% of the total dh/dt in the percolation zone.

There are two problems with the treatment of firn compaction here. The first is an extremely simple refereezing scheme: the simplest possible: a ratio of 0.6 of the normalised SMB. As Reeh points out and as is discussed here refreezing can have a dramatic effect on densification rates as does the overall energy balance of the snowpack. The second problem is the data used to force the firn compaction model. This comes from the HIRHAM RCM. This is a high resolution model but resolution is not everything and a small bias in the energy balance or accumulation at the surface may have a big impact on melt production and surface albedo respectively. I was unable to find any information, either in this paper, or in the cited reference on how well HIRHAM reproduces the climate and, therefore, how suitable it is to drive the compaction model.

A very minor related issue to this is the choice of ELA (line 22 p 2121) where a polynomial parametization is used but if HIRHAM provides a reliable SMB field then it would be far preferable to use the RCM estimated ELA. Indeed, it would be interesting to see how close the HIRHAM snowline is to, say, the end of summer snowline from MODIS although I accept that this is beyond the scope of this study.

Two lines earlier (line 18, 2121) an ice density of 900 kg m^{-3} is used. This is less than the standard accepted value for naturally occurring ice which is 917?

6 Additional elevation changes.

It is interesting to see this calculation in full for the GIA impact on mass change estimates, which confirms that it is insignificant but there are other processes not included in this study which may be significantly more important than this such as subglacial melting. A melt rate of 1mm/yr everywhere would result in 1.7 Gt/yr of mass loss from this source. Parts of the ice sheet are frozen to the bed but elsewhere melt rates may be as high as 15 mm/yr [*Fahnestock et al.*, 2001].

7 Mass balance of the GrIS

I would prefer to see this section removed. First the "comprehensive error analysis" relates to random errors associated with sampling and interpolation. It does not include a discussion of errors of omission nor systematic errors associated, for example, with the firn compaction correction and the implicit assumption that all the elevation changes are SMB driven, which is clearly a gross approximation.

Later on (line 20, p 2127) it is stated that "Modelled surface densities are used to convert volume change into mass balance" but earlier it is also stated that (p 212) "Depending on the assumed density of the volume changes the firn correction decreases the mass loss…". These two statements are not really compatible. If the former is true then the latter isn't necessary because it has been modelled but, as is suggested by the latter, the surface densities aren't really modelled or known. What is modelled is the possible impact of firn compaction on the measured volume change, which is something different.

Because of these issues, and the most serious, which is that ice dynamics are implicitly excluded from the analysis, a mass balance estimate from this study is inappropriate. If it is included at all then the various caveats **must** also be included.

One last minor but important gripe is that when discussing a mass or volume trend it is imperative to include the epoch at the same time because part of the issue around inconsistencies in the literature relate to comparisons between different epochs.

References

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