The Cryosphere Discuss., 4, C1137–C1147, 2010
www.the-cryosphere-discuss.net/4/C1137/2010/

The Cryosphere Discussions

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 #1

Received and published: 19 November 2010

1 General comments

This is a comprehensive and valuable study to the use of elevations measured with ICESat in order to compute the elevation/volume change rates and mass balance of the Greenland ice sheet. Much effort is put into each separate component/correction inclusive the contribution of its associated uncertainty to the total error budget. Especially, the work on various processing strategies of the ICESat data set is useful. Nowadays a number of estimates of the elevation/volume change rates and mass balance of the Greenland ice sheet based on this data set are published, which differ more than could be expected from the reported uncertainties. Beside the different time

span of the data used, all are based on various assumptions, processing strategies etc. This makes comparison of the various estimates difficult and also it becomes hard to quantify the impact of a particular processing strategy. Unfortunately the authors did not implement all distinct processing strategies published so far (about 4), which would have increased the impact of this study. Instead, some variants are implemented that differ however significantly from the published methods.

In any case, the most critical issue for obtaining reliable estimates of the elevation/volume change rates based on ICESat data is a representative sampling of the ice sheet, which is limited due to technical problems with the lasers onboard the ICESat satellite. Consequently, method M4 will never win the match with the other 3 methods so comparing this method with the others is, to my opinion, superfluous. For the remaining methods, the match is decided by the applied method of editing the estimate dH/dt, which is based on their associated variances. This, to my opinion wrong way of editing, makes the assumptions required and the way how the errors are propagated crucial to assess the performance of the 3 methods. Unfortunately, the manuscript contains no information about these topics.

In general, the manuscript is well written, although some parts need to be restructured and sometimes some more explanation will be helpful. Suggestions for improvements will be discussed in the next section.

2 Specific comments

2 ICESat data

The manuscript lacks a brief overview of what is measured and how this measured signal can be decomposed into various components. Now this information is hidden in the introductions of sections 2-6. I propose to bring it all together in one short section and add to this section also the components, relevant to the mass budget, that are not in the signal at all (omission error), like subglacial melting (as pointed out in the comment of Jonathan L. Bamber, 14 Nov 2010).

2.1 ICESat data pre-processing

- You use the IceSvar parameter to remove observations for which the misfit between the shape of the return signal and a Gaussian fit is large. First of all, the IceSVar parameter is the standard deviation of the difference between the functional fit and the received echo and NOT the difference itself. Furthermore I am wondering about the criterion of 0.04V, did you define it yourself or how did you derive it?
- Currently, it is hard to assess the impact of the various parameters/flags used for data editing. It makes sense to add in table 1, not only the total number of points before and after data culling, but also the number of measurements removed by application of a certain criterion.
- Multiple peaks can also be caused by topography in the illuminated footprint area.
- The authors state that: "... these thresholds result in a satisfactory size of crossover error." Where are these results and what they call satisfactory?
- In the GLA12 data product, only a rough guess is available (i_surfType) about the surface type. Therefore it makes sense to add how you exclude measurements that are not located on the ice-covered areas.

3 Methods for deriving surface elevation changes

- On page 2108 line 15-16 you mention only 3 contributions (terrain, seasonal variations and secular trends), but obviously there are more, see my comments above (2 ICESat data).
- On page 2108 line 17-20 the authors state the complications in deriving dH/dt are due to the presence of a cross-track slope. I do not agree, the problem is that the measurements are not repeated. Any separation between 2 successive measurements introduces a slope component, which can be decomposed into along-track and cross-track slope.
- "M1–M3 are along-track analysis and are all set up to estimate dH/dt at a 500m

along-track resolution." The footprints are separated 170 meter. Why you choose 500m and what if you change this to 1000 meter which would make the comparison between M1 on the one hand and M2, M3 on the other hand more fair (see first comment some generic comments for method 1-4)?
- Finally you mention that dH/dt values associated with a large variance are not used in the mass balance calculation. I will comment on this later, but I would keep this statement more general, i.e. that you use the estimated variances to edit your estimated dH/dt. This avoids the question what is large at this stage.

Some generic comments for method 1-4

- The basic assumption behind methods M1 and M3 is that the terrain can be described by a tilted plane. However, for M1, this assumption is made for an area of 1 by 1 km (the resolution of the used DEM), while for M3, this assumption is done for the 500m along-track segment. Obviously the unresolved contribution of slope is larger in M1 as it is in M3, which should be reflected by the associated uncertainties of $\triangle H^{M1}$. Depending whether you have taken the uncertainty of the subtracted DEM into account, this will affect the estimated variances and I guess they will be larger for M1 as for M3. Consequently, the number of estimated dH/dt values that will be removed afterwards will be larger. And especially this number is used to decide which of the methods performs best, see end of section 3.5. In other words, by this setup of method 1-3, M1 will behave always worse than M2 and M3.
- In general, the authors do not provide any information about the way how the errors are propagated. Especially I am curious to know what they assume for the uncertainty of one single observed elevation, was it the same value all over the ice sheet?
- The authors do not provide the results of their error propagation, i.e. the estimated variances. Since they use it later to edit the estimated dH/dt, it makes sense to add the plots of the estimated variances/standard deviations of dH/dt along with the estimated dH/dt itself.

- For M1-M3 I am wondering what the authors do at those locations where an ascending track crosses a descending one. At those locations you will have more observations. Is this exploited by the authors or not?

3.1 Method 1

- The used DEM is generated from the first seven operational periods of the ICESat mission. Roughly speaking, you could say that the reference epoch of this DEM equals the average time (center epoch) of those 7 periods. However, locally, this is not the case, since you will not have at each location data from each campaign. Therefore, the derived $\triangle H^{M1}$ will refer to different epochs depending on the location of the ice sheet. This confusing issue should be explicitly mentioned as a drawback of this method.
- Basically in M1 you subtract a low-resolution DEM from a high-resolution data. Indeed, since not all the topography is removed, the residual topography will propagate to the estimated dH/dt. However, is the uncertainty of the subtracted DEM added to the noise variances of the data? This is relevant since you finally use the variances of the estimated dH/dt in order to edit your derived set of dH/dt.
- Since you estimate dH/dt in 3 steps (i subtract DEM, ii compute mean $\triangle H$ for each ICESat campaign, iii estimate parameters using equation 4) I just want to be sure you propagate the errors correctly. So did you in i took the uncertainty of the used DEM into account and in ii did you compute the variance of the mean?
- A minor detail. In ii you compute the mean $\triangle H$ for each ICESat campaign. Since the time span of some campaigns is about to be 2 months, you might have more than 1 repetition of a track in 1 campaign, however due to various reasons (e.g. data culling) the number of measurements inside an along-track segment might be different per repetition, which will affect the estimated mean. I think you should account for this, by computing a kind of weighted average, i.e. average the averages for each repetition.

3.2 Method 2 - The description of this method needs to be improved. Especially I am

wondering how you derived dH/dx and dH/dy, are they estimated? Is it exactly the method presented by Pritchard et al. (2009), which is extensively discussed in the supplementary information of this publication? If so, you also need to clarify how you deal with the differences in timing of the measurements.
- You state that for each segment a reference surface is created, does this means that for each segment different tracks can be used to create this surface?
- Also here I am very interested in how you propagate the errors. Since I am not sure how dH/dx and dH/dy are derived I wonder what you do with the associated uncertainty in dH/dx and dH/dy.
- "This ensures that both the seasonal signal and the actual change in elevation ..." What do you mean by the actual change in elevation in this context?

3.3 Method 3

- Only for this method, the error propagation is trivial, since you estimate the parameters in one single step. I am only asking myself whether the estimated variances obtained with this method will benefit from the fact that you increased the number of unknowns.
- I do not understand the statement: "This method is sensitive to track geometry, since the method assumes that the H dependence in x,y and t is independent. For certain track constellations this will certainly not be the case." Do you mean that this method is sensitive to the orientation of the tracks in the terrain? Probably you can add an example of a track constellation to clarify what you mean.
- In equation 7 you use both t with a bar and t without bar, is this correct?

3.4 Method 4

- I propose to remove this method at all, since it will never provide you with a representative sampling of your signal, which can be concluded on beforehand.

3.5 Elevation change results

- This is the most critical issue in the comparison of the 4 methods. Since you removed all estimated dH/dt if its associated variance > 6 m^2 (I guess the unit is wrong?), the performance of the method depends on the way you propagated the errors. Beside that, I am afraid that along with spurious estimates, you also remove a part of the signal. This is already shown in the experiments of Slobbe et al. 2008 who used a n-sigma thresholding procedure. Furthermore, it is suggested by the comparison of the number of dH/dt estimates with variance below the threshold and the estimated volume change rates. In increasing order, for both you will get M4, M2, M1 and M3. Note that the difference in the number of outliers among methods 1-3 is large, e.g. between M2 and M3 $\approx$20.000! Since a representative sampling of the signal is crucial the impact of this step needs to be addressed in more detail. Probably you could also estimate dV/dt without editing your set of dH/dt. Since you used ordinary kriging to compute the smooth surface, you can easily propagate the derived uncertainties, which should be done in any case.
- I am wondering where the 'outliers' are located, but I guess mostly in the coastal region, where both signal and topography are large. Maybe you can add some figures?
- Why you use 6m^2, how did you derive it?
- If you are still convinced you need to edit your estimated dH/dt, I would propose not to edit your estimates based on its variance, but on the estimated dH/dt's itself or even better, using testing theory you can remove outliers during the estimation process itself. If you want to do it afterwards, you could try to use the smooth surface estimated to compute the volume changes to check for gross errors.

4 Deriving volume changes/4.1 Interpolation of volume changes

- You fit a smooth surface through the dH/dt estimates, to get coverage over the entire ice sheet on a 5x5 km grid. I wonder whether this is not too fine compared to the inner track spacing, especially in the southern part of Greenland. Anyway, close to the South you need strong extrapolation to get global coverage, compare fig 1 and 6.

- First you talk about ordinary kriging, but later on (2114 line 1) you talk about local neighborhood kriging, what did you use?
- I wonder how to get a smooth surface while the signal in the coast will be very irregular. Can you show the variogram, or provide its properties (correlation length etc.). Is the variogram estimated based on all data (coast/inland together)?
- In principle you assume isotropy, which is definitely not the case; I think you should comment on this.

4.2 Bootstrapping

- Your explanation of bootstrapping needs to be improved. You state: "Create a resample by drawing random samples with replacements from an original data set, . . ." What you mean by replacements, replacements in position? But how do you create these samples. Why not simply use your estimated variances to create 1000 realizations of the noise and use that to derive the uncertainty?
- "For method M1, M2, and M3 a resample is made by sampling between entire tracks contrary to individual dH/dt values, since these are highly correlated along-track." I don't understand what you mean here, can you explain this in more detail, what is highly correlated, the dH/dt values? But I don't see that in figure 1.

4.3 Volume change results

- Based on my previous comments I am not yet convinced about which method performs best. For me, this needs to be re-evaluated when more information about the methods and the error propagation is provided and when the impact of editing dH/dt is assessed.

5 Modelling firn compaction and surface densities

Since I am not that familiar with these topics, I will only give some short comments.
- You state that bm, us*(dS/dx) and ub*(dB/dx) are assumed to be constant but they do not appear in eq. 10. Why you neglect them?

5.3 Interpolated metric grid
- I would propose to remove the center figure of figure 3. Put a coastline in the other ones, change the color bar and add the location of station Nord.
- I cannot see the increased noise you mentioned. I even do not understand what should increase the noise.

5.5 Results of firn compaction and density modelling
- In the title of chapter 5 you talk about modelling surface densities. In this section it becomes clear you just assume different densities in different regions. This assumption is already used by Thomas et al. 2006. Probably you can add a reference.
- Above ELA you assume any elevation increase is due to accumulation. Why you assume this only above the ELA?
- Why the error in the linear fit in Fig. 4d is much larger than in Fig. 4e?

6.2 ICESat intercampaign bias correction
- In order to compute the intercampaign bias you assume an actual sea level rise of 0.3cm yr-1. Why you use a spatially independent slr, since DNSC08MSL is provided with a grid to correct for the slr?

7 Mass balance of the GrIS
- Why you derive an error estimate of the mass balance estimate again with the bootstrap method? Since you derived an error estimate for all separate components I suppose you can add these all together. From section 5.5 I see that even the error in the firn compaction is already larger than the one you derived with the bootstrap method (14-30 Gton/yr).
- There should also be an uncertainty due to the assumed densities; I think you should comment on this in the discussion.

8 Discussion and conclusions
- You error estimate of the firn compaction is not consistent with the one presented in section 5.5.

- For comparison with other studies I propose to add a figure or table that clearly indicates the estimated rate + uncertainty and timespan. Also more people presented an estimate, e.g. Luthcke et al. 2006.
- Comparison with Slobbe et al. makes no sense, i.e. beside different time span, Slobbe et al. did not correct for firn compaction, the elastic uplift correction and the intercampaign bias. Finally, the used methodology is different and unfortunately this method was not implemented in this study to compare it with other methods. Therefore, the statement: "we believe that we have improved the application of ICESat data to estimate the total mass balance of the GrIS, by using a novel approach including firn compaction and density modelling." does not make sense. In any case for a fair comparison with Slobbe et al. you should compare the estimated volume change rates before application of the intercampaign bias, note that these differ significantly.

3 Technical corrections

Abstract line 10: don't say, we find firn dynamics and surface densities to be important factors. They are important!

Abstract line 19: This result is in good agreement with other 20 studies of the Greenland ice sheet mass balance. But not with all of them!

Introduction: In the outline of the manuscript I miss what you will discuss in section 6.

2 ICESat data line 8-9: I see what you mean but this needs to be clarified.

3 Methods for deriving surface elevation changes (2108 line 15): "Thus the observed elevation difference between tracks contains contributions from terrain, seasonal variations and secular trends." Since the latter two are always in, change this to: Thus beside seasonal variations and secular trends, ...

5.1 2117 line 14: Add symbol for mass balance.

5.5 caption fig 4, first line contains two times the.

4 References

Luthcke, S. B., Zwally, H. J., Abdalati, W., Rowlands, D. D., Ray, R. D., Nerem, R. S., Lemoine, F. G., McCarthy, J. J., and Chinn, D. S.: Recent Greenland ice mass loss by drainage system from satellite gravity observations, Science, 314, 1286–1289, doi:10.1126/science.1130776, 2006.

Slobbe, D., Lindenbergh, R., and Ditmar, P.: Estimation of volume change rates of Greenland's ice sheet from ICESat data using overlapping footprints, Remote Sens. Environ., 112, 4204– 4213, doi:10.1016/j.rse.2008.07.004, 2008.

Thomas, R., Frederick, E., Krabill, W., Manizade, S. Martin, C., 2006. Progressive increase in ice loss from Greenland, Geophys. Res. Lett., 33, L10503.

Interactive comment on The Cryosphere Discuss., 4, 2103, 2010.