We appreciate the thoughtful and detailed reviews that the five referees have provided. The main purpose of this manuscript, and the main reason we submitted it as a brief communication, was that we wanted to highlight and demonstrate the importance of one specific issue related to deriving volume changes from radar altimetry, namely the slope-induced error, which is often overlooked or corrected for incorrectly (i.e., without relocating the radar footprints). We noted that at various instances the referees refer to other steps in the processing chain, which we did not intend to discuss in this manuscript. Below we respond to all the issues raised individually, for clarity also the reviews themselves are included. Our response is printed in italic font, changes in the revised manuscript are printed in bold font.

Response to review of reviewer #1

Major comments

a. The authors state it is very important to use contemporaneous estimates of slope because a small error in slope would cause a 10% error in horizontal relocation. However the crossover clusters contain data spanning 3 years, 2003-2006 and the slope grid used was published in 2001 so it was calculated with data several years previous to the ENVISAT data used. They should say something about what time period they consider to be contemporaneous and why that is acceptable.
We agree that errors and changes in the surface slope can affect the slope correction. Based on a revised sensitivity analysis (see reviewer #2) we revised the statements about the importance of slope errors. The objective of our manuscript was not to provide a definitive volume change estimate for Jakobshavn Isbrae, but to illustrate the importance of the slope induced error.

Nevertheless, errors from the DEM will only have a small effect on such a volume change estimate because i) errors in the DEM are typically spatially correlated, reducing the effect on slope (e.g., neighbouring pixels will probably have an error in the same direction, so the error in the slope will be relatively small); and ii) over regions where slopes, and therefore also errors and temporal changes in slope, are large, there are no valid radar altimetry data so any slope correction will hardly be affected by slope errors.

b. Please define the exact region over which DH/dt is calculated. In Fig 3 only Envisat dH/dt values between -2.0 and 1.0 are shown, what happened to the values < -2.0. In the result discussion it says the authors use all the data to within 3km of the grounding line. The transect in 3b shows ENVISAT values lower than -2m/yr. What happens to the correlation if all the clusters are included in 3a? Or were they used but not shown in 3a?
*In Figure 3A all available Envisat data points were used and shown - the minimum value is about -1.3 m/yr. As can be seen in Figure 1, however, there is only one data point located within the region enclosed by the 300 m/yr velocity contour that was used for the transect. In Figure 3b we did not state that the transects show interpolated dH/dt. This probably caused the confusion. We modified the caption ("**...(B) shows interpolated dH/dt...**"), and in addition, added asterisks to indicate the location of the actual data point. We also added this to the caption: "**..., and the asterisks indicate the (corrected and uncorrected) location of the only Envisat measurement in this area**

***(Figure 1).**" Interpolated dH/dt can be much lower than measured dH/dt because of the kriging method (kriging with external drift) that we used.*

c. I think more thought should be given to the effect of the up to 40km orbital altitude difference between the crossing passes. The authors state that it only causes a 5% error in the horizontal distance correction. Assuming a horizontal relocation of 10km, a 5% error (due to 40km difference in orbital height) over a 1 deg slope would translate to 500m horizontally with an 8m vertical displacement. Obviously the orbital differences aren't always 40km but even 10km is not at all unusual and that would still give several meters of elevation difference. This error may average out, but the authors should show how to support their results.
Again, we agree that orbit errors will add noise to the correction and we now mention this (along with other causes for noise) in the revised manuscript. However, there will not be a bias in the orbit but only random error and thus it is safe to assume that the correction for a dH/dt based on the entire time series cancels out. Also, as demonstrated the effect on the slope correction will be small.

d. For the discussion on volume change, the authors do not include the value for the ATM/ICESat data, but only state that it is larger. I think they should include the value so the reader can see how far off it is from the ENVISAT numbers. Including the ATM/ICESat number would give a qualitative value of how close it is to the smaller footprint results that do not have a slope-induced error.
We added the number for ATM/ICESat (-19.6 km^3 yr^{-1}) and calculated volume change with respect to this number (see response to reviewer #2). As could be expected from Figure 3, the difference in volume change between ATM/ICESat and Envisat is quite large as the area with large thinning rates is much larger in ATM/ICESat, for reasons that are explained in the text.

<u>Minor comments</u>

a. Figure 1, cannot see the velocity contours. Perhaps use thicker lines. We *have thickened the contour lines for velocity.*

b. Line 1 page 162; suggest measured "to" an upslope location.
We assume the reviewer means page 161 here. However, we believe "measured AT a location" is more appropriate here than "measured TO a location".

c. Figure 2 – Please increase the font used for the axes titles in B and C and put units on the color scale.
Changed as suggested.

d. Uses different nomenclature in Figure 2 than in the text – line 24 p 162, E vs Rc – make them consistent
With Rc and E we mean two different things: R refers to an exact range measurement for absolute elevation, that is corrected to Rc using the three correction methods (direct, relocation, and intermediate). Figure 2 and the corresponding part of the text are consistent in using R and Rc. For our correction, we do not have such an accurate measurement, but we use the (uncertain) satellite altitude E, which is on average 800 km but fluctuates between 780 and 820 km. We state, at page 162 line 25, that the "...E is the

satellite altitude, equivalent to Rc in Fig. 2a, ...". To clarify, we added to this sentence: ***"...equivalent (but not necessary equal) to Rc...".***

e. Figure 3 – increase the font on the axes labels.
The font size has been (slightly) increased.

f. P 164 line 7 Isn't the correction to dH/dt a change in elevation and not elevation. Why are units in meters and not meters/year?
That is correct. We changed this sentence to: ***"..sometimes with elevation change corrections of several metres per year."***

Response to review of reviewer #2

<u>Major comments</u>

(A) To say it with some exaggeration, the study investigates SRA errors in observing a signal that is largely unobserved by SRA. More specifically, most of the volume loss occurs in the fast-flowing part within the 300 m/yr velocity contour. This is exactly the area where virtually no altimetry data are available from the employed SRA crossover analysis. (More precisely: exactly 1 crossover point is available.) The SRA-based elevation rates in the high-velocity area are therefore almost purely the result of an interpolation (or rather extrapolation?) using an approach of kriging with velocity information as an external drift, with reference to a manuscript under review. Thereby, high elevation rates in the fast-flowing part are deduced from much lower elevation rates in the slower parts. Without doubt, it is an attractive idea to estimate elevation rates in the fast-flowing part by a combination of flow velocity data, assumptions about the relationship between flow velocity and elevation rates, laser altimetry data (used, at least, for validation purposes), and SRA data outside the fast-flowing part. However, for such an estimate the effect of the SRA slope correction is a puzzling interplay between the slope correction (relocation) itself and the effect of the interpolation. The methodology outline and illustration in Section 3 does not cover this interplay but purely refers to the case of an area well-covered by SRA. Without more insight into the interpolation method, the reader has difficulties to assess the general significance of the results. Figure 3b illustrates this dilemma. This figure shows SRA results confined to the area within the 300 m/yr velocity contour, where there is just one single SRA observation, located at the upper end. I propose that this issue should be discussed, at least. I would also find it helpful to present a separate analysis for the area that is really sampled by SRA, for example the area between the 100 m/yr and the 300 m/yr velocity contours.
We agree with the notion that interpolation plays an important role obtaining the eventual volume change. However, the purpose of the paper is to illustrate the effect of slope-induced error and not come up with a definite volume change estimate. To do that, altimetry data will have to be interpolated and regardless of the interpolation method the slope-induced error needs to be corrected for, because the location of the measurements is important for any interpolation. We cannot elaborate here on the specific interpolation method we used due to space limitations. However, the manuscript describing this is in review.

In the revised manuscript, we added a volume change calculation that is based on the area that is entirely sampled by SRA (between the 100 and 300 m/yr velocity contours; see also our response to (B)). The numbers are essentially the same as the calculation for the entire area within the 100 m/yr contour, indicating that the effect of the slope correction is relatively robust and does not rely on the interpolation.

(B) The quantification of the slope correction effect ("32%") is relative to the uncorrected (biased) estimate. In fact, both SRA-based estimates appear to be biased low. It would be more informative to relate the slope error effect to a best estimate of the true volume change. While the authors have computed such an estimate from the laser altimeter data (p. 164, line 26), they do not quote it. From Fig. 3b and independent sources (Joughin et al. 2008, Khan et al. 2010) I would guess that such an estimate is on the order of -20 km3/yr. Then, a more objective and more informative quantification of the slope correction effect would be of the following type (with fictive numbers): "Without slope correction, the error of the SRA-based volume rate is -57% of the signal. With the slope correction, the error reduces to -43% of the signal." An analogous (and probably more satisfying) statement could be formulated for the area that is really sampled by SRA.
We agree that statistics with respect to the (ATM/ICESat) reference would be more informative. We calculated the numbers suggested by the reviewer (which were remarkably accurate) and replaced the original 32% by the newly calculated numbers. For the area enclosed by the 100 m/yr contour, the ATM/ICESat volume loss is 19.6 km3/yr. Therefore the error reduces from -56% to -42%. For the area between the 100 and 300 m/yr contours, which is nearly completely sampled, the error reduces from -55% to -44% (ATM/ICESat volume loss is 6.6 km3/yr). For the area enclosed by the 300 m/yr contour, which fully depends on interpolation, the error reduces from -57% to -41% (ATM/ICESat volume loss is 13.0 km3/yr). The numbers, therefore, do not change very much depending on the sampling. We included the numbers for the area between 100 and 300 m/yr in the results section as well, and changed the conclusions and abstract accordingly based on the numbers for the area enclosed by the 100 m/yr velocity contour.

Minor comments

(1) Abstract: Revise the statement that the correction "increases elevation change rates by several metres". It should read "meters per year" and "up to several meters", I would suggest. Also, as discussed in (A), it has to be clarified that this is not the effect of the correction on the observation itself, where the slope error cancels out. In the same spirit, one might re-consider the title. I would prefer "volume change estimates" instead of "elevation change estimates". Then it would be clear that an interpolation is involved.
Both title and abstract are changed as suggested.

(2) 160:20 (meaning page 160 line 20): the quoted 14 km displacement depends on the height of the specific satellite, which is not mentioned.
That is true. We added "...and a satellite altitude of 800 km..." to line 160:18.

(3) 162:5f: Clarify whether the velocity fields were derived within this study or taken from an external source. Similarly, in 161:9: Are the elevation change rates provided by Li and Davis or derived by yourself?
We slightly altered the formulation in both cases to make clear we used velocity from an external source (I. Joughin) and used cross-over clusters from Li and Davis from which we derived the elevation change rates ourselves.

(4) 162:15f: Clarify the explanation. Currently, only those readers will understand it who already know the three slope correction approaches. For example, R_c is used with different meanings. The formulation "R_c: the closest point to the satellite" seems to suggest that R_c denotes a point, etc.
Space limitation prevents us to significantly extend the explanation of these three methods, hence the citation to Bamber, (1994). We agree with the inconsistent use of R_c and reformulated the explanation of the relocation method: "The second method, the relocation method, corrects R to R_c, where R_c is now the range to the point closest to the satellite (now Rcos(\alpha)),..."

(5) Fig 1: velocity contours should be marked and annotated more clearly because they are important for the further results.
The line thickness of the contours has been increased to increase visibility, the values are explained in the caption.

(6) Fig. 2b,c: Color scale needs units.
Changed as suggested.

(7) 163:2 "sensitivity to slope angle is larger": Clarify, how you compare the sensitivity to satellite height to the sensitivity to the slope error. The chosen numerical example compares the effect of a 10% slope difference with the effect of a 4% height difference.
We agree that this statement was wrong. We recalculated the sensitivity to slope with a 5% uncertainty (800±20 km is a range of 5%), so we tested slopes 1±0.025 degree, and found that the range in horizontal displacement of 5%. The sensitivities to orbit errors and slope are thus about equal and we changed the statement accordingly.

(8) 163:9: What is meant by crossover location: The nadir location or the relocated one?
We replaced 'cross-over location' by 'nadir location'.

(9) 164:5: By what borders is the study area defined?
We arbitrarily selected a study-domain encompassing the fast-flowing part of Jakobshavn glacier. It is roughly bounded by the 100 m/yr velocity contour (Figure 1).

(10) 164:6f: Fig. 3a illustrates corrections on elevation rates (not elevation) in meters per year (not meters), right? Is it justified to say that these corrections are sometimes several meters (per year)? That is, does the correction exceed 2 m/yr in any case? From the figure one cannot judge. It might be nice to identify the pairs of red and blue dots that belong together.
The method indeed corrects elevation change rates, we therefore changed "several metres" to "several metres per year". The largest correction visible in Fig. 3A is almost 4 metres per year (at -0.1 m/yr on the x-axis), and another

one is about 2.5 (at -1.4 m/yr on the x-axis). We therefore feel the statement is justified. Pairs that belong together can be identified in most cases (it is somewhat difficult in cases of data clustering) because they have the same X-coordinate.

(11) Fig. 3b: Explain the dash-dotted line. If this shows the flow velocity, then there might be a problem with the right ordinate axis. I would have expected a value of about 300 m/yr at 80 km from the grounding line, but it is about 1.2 km/yr, instead.
There was indeed a mistake in the range of the right ordinate axis which has been corrected (the velocity at 80 km from the grounding line is indeed about 300 m/yr).

(12) Fig. 3c: explain contour lines again
Contours and values are explained in the figure caption.

(13) 164:7: It could be formulated more clearly that reason (i) is a cause for noise in the SRA data while reason (ii) is a cause for noise in the ATM/ICESat values.
We changed the formulation to: ***"...considerable noise in the corrected scatterplot, because i) the correction only corrects Envisat data for the footprint-average slope and not for smaller scale undulations, and..."***

(14) 164:17: The text states that the correction effect at 10 km distance from the grounding line is about 4 m/yr. The Figure 3b, in contrast, shows about 1.6 m/yr.
These numbers were indeed mistaken. We changed them to "about 2.5 m/yr at 5km" from the grounding line.

(15) 164:19 and Fig. 3b: This sentence might be confusing, since (i) it states that results close to the grounding line are not used but (ii) it discusses the curves at the extreme left of Fig. 3b in terms of values "close to the grounding line". It might be an option to show the curves in Fig. 3b just starting from 3km on the abscissa.
We agree with this. Fig. 3b is now cut off at 3 km from the grounding line, and the text "hence dH/dt values increase close to the grounding line" has been removed.

(16) 164:27 most readers will be interested in the numbers from ATM/ICESat.
We agree and this was also pointed out by the other reviewers. We now mention the number and calculated volume change for both corrected and uncorrected elevation change data with respect to this ATM/ICESat number (see B in the major comments above).

(17) 164:26 The sentence suggests that residual errors of the interpolated SRA volume changes after slope correction are (purely?) due to surface mass balance changes not accounted for by the interpolation algorithm. For the moment, the reader has no substantiation for this assertion.
We didn't mean to suggest that this is the only reason for the difference but wanted to provide a possible explanation. We changed the wording to ***"One explanation for the much larger volume changes from ATM/ICESat compared to Envisat is the better sampling...".***

(18) 166:12: reword "ice sheet mass loss from ice sheets".
We removed "from ice sheets".

Response to review of reviewer #3

Major comments

I agree with previous comments about the presentation of the final conclusion. The most important is not to know the difference in the retrieval of volume change with and without this correction. The most important is to know the reduction of error in volume change deduced from radar altimetry with respect to ATM or ICESat (e.g. clearly write volume changes difference between ATM/ICESat and Envisat without correction is xx, and volume change difference between ATM/ICESat and Envisat with correction is reduced to yy…).
We changed this in revised manuscript; more information is provided in the response to reviewer #2.

Also, the use of RA-2 cross-over and not the whole along-track data yields to a poor sampling that strongly penalizes RA-2 compared to ICESat. Such a study should have used along-track data.
We agree that sampling is an issue, and probably this contributes to the large difference in volume change from Envisat with respect to ATM/ICESat. However, the slope correction issue remains and although the difference with ATM/ICESat maybe smaller we do not think the effect of the slope correction would be very different if we would have used along-track data. In any case, the Envisat data that was available to us was in the form of cross-overs.

About methodology, I do not understand why they estimate the displacement via the surface slope in order to find the closest point, instead of directly look for this point on the topography map. This would allow to take into account surface curvatures and kilometric scale topography features, the closest point would be determined more precisely than with average slope alone.
We chose to use the surface slope because other slope-correction methods that we are aware of (Brenner, 1983; Bamber, 1994; Remy, 1989) use surface slope for the correction. Another reason for this is that elevation changes are measured over multiple years and it is not always easy to use a completely contemporaneous DEM (see also response to reviewer #1). Directly using topography greatly increases sensitivity to errors and changes in surface slope. Because a DEM often largely depends on interpolation (at least the DEM that we used in this study), we do not think it is sufficiently reliable to select the highest location in each radar footprint and choose to use spatially averaged slopes.

However, for me, the greater problem that should be addressed is the problem due to the antenna aperture versus the surface slope of the chosen area. The half antenna aperture of the Envisat RA-2 is 1.35°, (the gain is $(3.3°)^{-2}$), meaning that in case of surface slope of 1°, the energy backscattered from the impact point is 4% of the energy coming back from nadir. This induces two problems. First, the waveform is strongly distorted with a long leading edge and is thus very sensitive to any change in snow-pack properties or short-scale topography features and retracking (probably both ice-1 and ice-2) gives elevation with a poor precision.

This is true and the reason that there are hardly any valid Envisat cross-over clusters located in the fast-flowing, and steep-sloping areas - slopes are too steep for successful retracking and/or precision is too low. All clusters that were used here are located in areas where slopes are smaller than 0.5 degrees. We assume retracking was sufficiently accurate to obtain a valid dH/dt and small-scale topographic features in those areas do not play an important role.

Second and more important, with a surface slope of the order of magnitude of the half antenna aperture, the height retrieved from the distorted waveform does not correspond to the theoretical impact point (closest point). In general the up-slope shift is reduced and the mean elevation corresponds to a spot delimited by a convolution between antenna pattern gain and topography. This can be demonstrated with a dual frequency altimeter. Over steep areas, the lower frequency (C-band for Topex or S for Envisat) gives elevation higher than Ku-band because the antenna aperture is greater (and the energy backscattered from impact point is greater). See for instance Remy, F, Legresy B, Bleuzen S., Vincent P. and J.F. Minster, 1996, Dual-frequency Topex altimeter observation above Greenland, J. of Electromagnetic Waves and Applications, 10, 1505-1523. To be optimal, a waveform simulation must be performed with the help of the small-scale topography and antenna gain pattern in order to measure the height retrieved with a given retracking (maybe for a future paper...).. For now, the authors should acknowledge the problem. For me, this explains the poor contribution of the correction for the steep part of the chosen profile (see between km 10 and 40 on Fig 3.b). The correction only slightly reduces the difference with ATM/ICESat. I suggest to superimpose the surface slope in Fig 3.b (added or instead of velocity).
As was also mentioned in our response to the previous issue, there are hardly any cross-over cluster locations over steep terrain where successful dH/dt retrieval was possible. In the section between 10 and 40 km, therefore, no data are present and the shown profiles are in fact based on extrapolation (see also our response to reviewer #2). We think this fact is more likely to explain the large difference with ATM/ICESat as is mentioned in the text (end of the "Results" - section) than slope correction inaccuracy. In the revised paper, this sampling issue is stressed by adding the actual data point (there is only 1) to Fig. 3b. Also, we added the surface slope profile to Fig. 3b.

<u>Minor comment</u>

The profile shown on Fig 3.b could be shown on Fig. 1
The transects shown in Fig. 3b are average values calculated in north-south direction, where only the area enclosed by the 300 m/yr velocity contour is taken into account. The contour is included in Fig. 1.

Response to reviewer #4

<u>Major comments</u>

From Fig. 3b, even corrected dH/dt show large discrepancy with the ATM/ICESat dH/dt from a location that is about 45 km away from the grounding line, where the ice velocity starts to increase rather exponentially. However, the reason is not explained in the manuscript. The difference between them is shown to be as

large as 4 m/year, a large number considering that basin-GH in West Antarctica is observed to have 2 m/year of dH/dt recently (Lee et al., 2012).
We agree there is large discrepancy between ATM/ICESat and the radar altimetry. The main reason is sampling: the fast-flowing region is poorly sampled by Envisat, whereas ATM/ICESat covers that area densely. In fact, as reviewer #2 remarked, there is only 1 Envisat cluster inside the 300 m/yr velocity contour, so interpolation plays an important role. Our interpolation algorithm deals with Envisat's poor sampling This is to some extent compensated by our interpolation algorithm, which inter(extra)polates elevation changes based on the spatial velocity gradients. Unfortunately the manuscript where is this is explained in detail is still in review, space limitation prevents us from expanding it here. However, only ice dynamics are taken into account, while ablation also can amount to several metres a year. The fact that this is captured by ATM/ICSat and not by Envisat is an important reason for the discrepancy. This is explained in the manuscript (page 164, lines 26-29), and has been slightly expanded in the revised manuscript.

Elevation change rates are indeed large, certainly when compared to Pine Island glacier in Antarctica. However, dH/dt rates of 15-20 m/yr are not uncommon on Jakobshavn Isbrae (e.g., Joughin et al., 2008b).

It is speculated that some of the error could be due to the DEM accuracy near the coastal region because the accuracy of radar altimeter measurements (used to generate the DEM) over the rough topographic surface is expected to be poor. In other words, some error budget analysis can be performed to show how the DEM error can result in the corrected dH/dt error. Although it may be out of the scope of the paper, some brief discussions about the retracking method may be included as well. What retracking has been used? Will different retracking method help to reduce the discrepancy with the ATM/ICESat dH/dt?
Errors in the DEM will indeed affect the slope correction to some extent but this effect will be limited for reasons explained in the response to reviewer #1.

We did not retrack the Envisat data ourselves but we used values from University of Missouri as published in Li and Davis, 2008 (full reference in the manuscript). More information on the retracking and processing can be found in: Davis, C., C. Kluever, B. Haines, C. Perez and Y. Yoon (2000), "Improved elevation-change measurement of the southern Greenland ice sheet from satellite radar altimetry", IEEE T. Geosci. Remote Sensing, 38, 1367-1378.

A full error budget analysis, as well as a full discussion on retracking methods, is indeed out of the scope of this paper.

<u>Minor comments</u>

1) Volume change number from ATM/ICESat should be included.
We included this number and also calculated the volume change statistics with respect to this number, as was discussed in our responses to the other reviewers.

2) Fig 1: please add explanation about ICESat tracks (maybe use different color?)
We changed the colour of the ICESat tracks to red and added this to the caption.

3) Fig 3b: the dotted line seems to be velocity profile, but a legend can be included.
Based on comments from reviewer #3, we added the slope transect to Figure 3B and added a caption for both slope and velocity.

Response to reviewer #5

Major comment

The paper is concise and presents a clear example of the potential effect of slope-induced error on altimeter data and derived estimates. However, all the analysis are restricted to one particular region, the Jakobshavn Isbrae Glacier, which makes it difficult to generalize the magnitude of this effect for any other region.
We indeed limited our analysis to one test-case only. However, poor sampling of outlet glaciers by radar altimetry is a common problem because outlet glaciers typically are steeply-sloping and narrow. Also, in general outlet glaciers are locations where elevation changes are largest (e.g. Pritchard et al., 2009). Although the numbers will change from case to case, the problem and methodology are valid for other outlet glaciers as well.

Minor comments

Page 160 line 18-20: Is this for a particular altimeter configuration? If so, which one (ERS-1, Envisat,...)? If not, it would be good to have a reference for this.
*These numbers depend only on satellite elevation, and most radar altimeters (Envisat, Seasat, ERS-1/2, ...) have an orbit altitude of about 800 km. The numbers follow from simple trigonometry (e.g. 800*sin(1deg)cos(1deg) = 14 km - the equations are provided in Section 2.)*

Page 163 line 3: The uncertainty in the slope angle is indeed crucial to the slope correction. How to have contemporaneous slope estimates (everywhere needed) independent of the altimeter measurements to be corrected? Need to expand a little bit the discussion about this.
As was also discussed in response to reviewer #1, while the slope correction is relatively sensitive to uncertainties in slope, the obtained volume change estimates, or dH/dts, will probably not be effected very much because i) errors and temporal changes in elevation are typically spatially correlated (i.e., resulting error in the slope will be smaller); and ii) in the areas where slopes and also errors/changes in slope are large, there are hardly any valid Envisat data locations, exactly because of these steep slopes. If possible, however, one should use a DEM that is contemporaneous or corrected using known dH/dt values.

Page 164 line 8-10: What about backscatter correction? This correction can effectively diminish the amplitude of the signal by a considerable amount (for example by 80%).
Backscatter correction is indeed another reason for noise between ATM/ICESat and Envisat, along with various other such differences (e.g. footprint size). We added, therefore, after reasons i) and ii), a ***reason "iii) various differences***

between radar and laser altimetry, such as footprint size, orbit errors, and the backscattering correction necessary for radar altimetry."
Processing prior to the dH/dt analysis included a backscatter correction and was done similarly to Li and Davis, (2008). More information can be found in: Davis, C., C. Kluever, B. Haines, C. Perez and Y. Yoon (2000), "Improved elevation-change measurement of the southern Greenland ice sheet from satellite radar altimetry", IEEE T. Geosci. Remote Sensing, 38, 1367-1378.

Page 164 line 10-12: How to determine in the "uncorrected" data set what data points are (or aren't) outliers? That is, if a robust fitting is used instead there are at least two points in Fig. 3a (far away from the fitted line) that would probably be treated as outliers, and the correlation would be much higher for the "uncorrected" data set.
We mention outliers at line 164/5, which is not really appropriate as we indeed do not remove or filter outliers. We therefore removed this and in the revised manuscript state that "Uncorrected values are generally corrected towards....". The entire dataset is only 23 points, so individual points are bound to heavily affect the correlation coefficients. Because these coefficients, and indeed this entire analysis, are not used for the correction or the volume change estimate, we think it is not appropriate to here remove outliers from the plot.

Page 164 line 29: What dynamical changes are we talking about here? From a stationary velocity field only the steady state dynamics can be represented. To distinguish changes associated, for example, to glacier surges one would need a representation of the velocity field for different periods of time (a time series), something that in most cases is not available.
With "dynamical changes" we mean dH/dt caused by ice dynamics. To clarify this we replaced ***"dynamically induced changes"*** *by* ***"dynamically induced dH/dt"****. To fully account for ice dynamics, one would indeed need velocity time series. However, the interpolation algorithm that is described in the referenced paper uses spatial velocity gradients, which is related to spatial gradients in dH/dt but not by a fixed relation. This means that the relation is constrained by the available dH/dt data and a given velocity gradient can produce different dH/dt gradients. The manuscript is describing the interpolation is still in review. it is outside the scope of this paper to elaborate on it.*

Page 165 line 3-4: By whom? I don't think this is the common practice. For example, some products such as the L2 IDRs from GSFC they come with a slope correction applied (see Brenner et al., 1983).
There is indeed a slope correction applied to the L2 GSFC product. However, and this is a very important point to make, they make use of the direct method (see http://icesat4.gsfc.nasa.gov/radar_data/data_processing/slopecor.php).

Because in this case measured elevations are corrected but not relocated, the net effect on dH/dt is zero (or only changes slightly with temporal changes in the slope). For dH/dt and volume change estimates, it is essential that measurements are relocated to their actual location (as is done by the relocation method). To emphasize this more in the manuscript, we changed the formulation in the Conclusion-section to: ***" …the dH/dt location is often ignored because the vertical error cancels out in repeat measurements, or the direct method is employed."***

Page 165 line 10-12: Careful when generalizing this statement. The slope error is mostly problematic at the margins of the ice sheet (where the steep slopes are), ice shelves and the smooth topography of the ice sheet interior do not suffer much from this effect.
We agree that the slope correction will not have a large effect in the interior. However, elevation, volume, and mass changes caused by dynamics and ablation are concentrated at the ice sheet margins in both Greenland and Antarctica (e.g. Pritchard et al., 2009), where the slope correction will have an effect. Perhaps Jakobshavn Isbrae is an extreme example, but on other outlet glaciers the same issues (steep slopes, poor sampling) play a role.

Page 168 Fig. 1: I would suggest drawing the velocity contours in white with the respective value on each contour. It would also be useful to have a circle representing the radar altimeter footprint (PLF) in real scale.
In the revised figure, the contours are white and labels are added. Although not intended, the size of the dots representing uncorrected Envisat data, are nearly exactly the size of the pulse-limited footprint (assuming a footprint radius of 1.6km (e.g., Fricker and Padman, JGR, 117, C02026, 2012).

Page 170 Fig. 3: All the figures need a larger font; the text in the axis is difficult to read.
The font sizes in the revised figures have been slightly increased.