Review of Melkonian et al., Cordillera Darwin Icefield: Small error bars probably do not capture the real uncertainties on the estimate of mass loss

SUMMARY
This is the third paper from this research group dealing with glacial mass loss in Patagonia Icefield (NPI, SPI and now CDI). This one (dealing with CDI) and the NPI paper also include surface ice velocity measurements. Elevation changes are obtained by fitting a linear trend to a time series of elevation measurements derived from SRTM and ASTER digital elevation models (DEMs), then integrated to get total volume change. [BTW, a similar multi-temporal DEM methodology has recently been published by (Nuimura et al., 2012) in Nepal and may be referred to]. Velocity maps are obtained by cross-correlation techniques applied to optical and SAR imagery. This is the first time that the mass budget of the CDI is published. Melkonian et al. found strong mass losses with a region-wide mass balance which is as negative as the one of the SPI (at about -1.5 m w.e./yr^{-1}).
The CDI is a poorly surveyed area and the mass loss reported here will contribute to the efforts of the glaciological community to better constrain regional and global mass loss from glaciers (and thus their contribution to SLR). I am less convinced by the added value of the regional velocity field but in the case of one glacier (Marinelli) it contributes to the understanding of its tidewater dynamics. The regional velocity field will also be a baseline dataset (available upon request?) to study future changes in ice dynamics.
Despite the fact that the methodology has already been published, I have some suggestions/questions regarding the processing of the data and the error analysis. The authors report on a strong sensitivity of their total mass loss to various assumptions and this is not reflected at all in the small (less than 10%) and purely statistical error bar. This is not acceptable.
This issue (uncertainty) excluded, my comments mainly aim at improving the paper and maybe making it a bit more "appealing" to the scientific community. Then, it will be a useful addition to the growing literature about glacier change in Patagonia.

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GENERALS COMMENTS

1/ Methodology & uncertainty of the elevation changes and mass loss.
Although presented (and thus peer-reviewed) in two previous papers, there are still some points that could be clarified/improved.

<u>Coregistration of the DEMs</u>.
ASTER DEMs share the same geometry as the corresponding ortho-images and thus the latter are coregistered with sub-pixel accuracy to a Landsat mosaic, which is fine. But the issue which is not addressed is then how good is the coregistration of the Landsat mosaic to the SRTM DEM? Coregistration of a 15 m image mosaic to a 90-m DEM is not an obvious matter and will lead to uncertainties which are not discussed. The methods of (Nuth and Kääb, 2011) could be used to verify the goodness of the coregistration of ASTER DEMs and the SRTM DEM.

We agree that the quality of co-registration between the SRTM DEM and Landsat GLS imagery is important. Tucker et al. (2004) and online reports by NASA and others indicate the coregistration was completed with "national means", i.e. classified procedures, probably involving the 1 arc second (~30 m/pixel DEM) instead of 3 arc second (~90 m/pixel) product. The results were tested, globally, rigorously, with ground truth measurements.

other studies (e.g. Fujisada et al., 2005; Berthier et al., 2010). We also visually inspect the difference between the ASTER and SRTM DEMs to look for systematic offsets near ridge lines that would indicate errors in coregistration. We think that our methodology is consistent with the state of the art (e.g. Berthier et al., 2010) and that future work should compare this methodology to Nuth and Kaab (2011) to further refine uncertainty estimates.

Maximum allowed thickening at high elevations.

By necessity the maximum thickening rate allowed is arbitrary. It is 5 m/a here; was 3.5 m/a in the SPI study and apparently not applied to the NPI case (?). Removing outliers using a non symmetric range of acceptable values may bias your results. I see two possible strategies around this issue: One, see (i) below, would permit to avoid such an arbitrary choice while the other (ii) would at least allow including it in the error bars. I hope you will be able to test them and report on the result.

The method suggested (i) is not applicable to our procedure for removing outliers as it is applied after calculating the dh/dt, while we remove outliers before calculating the dh/dt (see next response). We have made this more explicit in several places in the text to avoid this confusion. In other words, we do not place bounds on the allowed slope of the line (i.e. restricting the maximum thickening/thinning rate), but rather remove elevations that deviate from the first elevation (SRTM for 94% of dh/dt) by more than a certain amount per year. We use the SRTM DEM as a "reference" because it is not affected by clouds, unlike ASTER observations that are and are therefore more likely to be spurious. The difference between excluding elevations based on deviation from the first elevation before calculating the dh/dt and restricting the dh/dt rate without removing the bad points is quite large. Fig. 10 shows that if we included the excluded points (bolded red) were incorporated into the regression, we would have dh/dt values much different from the ones shown. We note that reviewers and editors of our previous papers felt that +3.5 m yr^{-1} and +5 m yr^{-1} are implausibly large and pushed us towards considering zero-thickening at all high-elevation sites.

We exclude individual ASTER elevations at each pixel when required in a non-symmetric fashion for the following reasons:

1) Coherency. Doubling the allowable deviation in the ablation zone from -30 m/yr to -60 m/yr produced no changes at the rapidly thinning zones at the fronts of Marinelli, Dariwn and CDI-08 glaciers. Allowing -60 m/yr deviation results in what we can only term "random splotches" of incoherent, extreme thinning that are not plausible.
   This is the same reason we restrict the deviation from first elevation to -10 m/yr in the accumulation zone rather than -30 m/yr. Allowing a deviation of -30 m/yr results in incoherent splotches of thinning, likely due to erroneous elevations rather than representative of real changes.

2) Plausible rates of precipitation and b-dot. We do not believe that thickening and thinning rates at the icefield should be symmetric. The thickening rate cannot exceed the maximum precipitation at high elevations. Thickening must be less than precipitation because of compaction and melt. Precipitation data for this region is sparse. We allow a maximum positive deviation from SRTM of 5 m/a based on accumulation rate estimates given by Koppes et al., 2009 (see figure 8a). They use NCEP-NCAR along with climate station data to

which is not seen visually in any of the images. This deviation would also capture the effects of low clouds. This has been considered further in Willis et al. (2012b). Maximum thinning rates can and do exceed thickening rates for many glaciers, and we are very confident that the thinning rates we give for the fronts of Marinelli, CDI-08 and Darwin are as high as indicated (we can see thinning manifest on imagery as bedrock becomes exposed). We particularly trust ablation zone measurements, as ASTER-derived elevations are more reliable towards the front of glaciers because exposed textured ice correlates better than the relatively featureless snow at high elevations. The glaciers are also generally flatter at lower elevations. In low-elevation areas where we do encounter cloud cover, clouds tend to result in higher elevations, so thinning rates are less affected by this source of error (which is the primary source of elevation error) than thickening rates. Our derived pattern of thinning shows a progression from more thinning at lower elevation to less thinning at higher elevation (which, as the reviewer suggests, is not an unexpected pattern of thinning). Were the thinning rates merely due to noise, we would expect them to be "incoherent", i.e. more randomly distributed throughout the ablation zone.

Added, abstract, second paragraph: "To avoid including spurious elevations (primarily from clouds) in the calculation of the mass loss rate, we exclude elevations from the regression that deviate by more than +5 m yr^{-1} from the initial elevation. Doubling the maximum allowed deviation to +10 m yr^{-1} is unreasonable as it exceeds precipitation data, but it does provide what we consider the lowest possible mass loss rate based on the ASTER and SRTM data for the CDI, -1.8±0.4 Gt yr^{-1}."

Added, section 2.2, second paragraph: "It is important to note that although these cutoffs are based on expected dh/dt, they are imposed on elevations at individual map points rather than to already-calculated dh/dt, which would yield drastically different results. … The high percentage of points with SRTM elevations makes our cutoff strategy feasible."

(i) A strategy to remove outliers and is to apply a n-sigma filter (typically n = 3 but you may play around with other values 2 or 4) to each elevation band (Berthier et al., 2004;Gardner et al., 2012). It is best if you apply it to each individual glacier (and not to the whole icefield) because it is reasonable to expect little variability within one elevation band on a single glacier whereas, and as you clearly already shown, different glaciers have different dh/dt curve as a function of altitude. It seems to me that such a strategy of excluding outliers would be less arbitrary than the "maximum thickening rate" strategy that you used.

We think the method of Gardner et al. (2012) that the reviewer is referring to is section 3.3.1 of their paper. Their n-sigma filter is applied after dh/dt has been calculated (actually a difference in Gardner et al., rather than a multi-temporal regression as we use). Our method excludes (pre-filters) elevations based on their deviation from SRTM before the dh/dt is calculated. Gardner et al.'s method is a post-filtering technique. We have made our methodology clearer in the text.

Even as a post-filtering technique the n-sigma method is not entirely suitable. For example, thinning at a glacier such as Marinelli is influenced by the dynamics of the glacier. Different parts of Marinelli that are at the same elevation can have different thinning rates due to an orographic effect, wind redistribution of snow, shadows, or locally varying ice dynamics.

Berthier et al. (2004) touch on this in section 2.4 of their paper: "shadowing and debris coverage on the glacier make the assumption of a similar behavior for all points at a given altitude only partly

dh/dt. To do so, a possibility would be to consider that your dh/dt distribution on glacier is the sum of the real signal and some noise (with probably nearly a Gaussian distribution?) that, in fact, you already determined on the ice free terrain (and quantified by the standard deviation around the mean elevation bias). I think the mean of all stable-terrain dh/dt values within the range [-30;+5] m/yr (respectively [-10;+5] m/yr) will give you a first order estimate of the systematic error that is induced by your non-symmetric cut-off strategy below (respectively above) the ELA.

Our pre-filtering does not introduce uncertainty, it removes obviously erroneous points from consideration when calculating the regression. Calculating dh/dt on off-ice areas using the same cutoffs as on-ice doesn't appear reasonable to us, given that we know the dh/dt on bedrock should be zero (discounting tectonic uplift). This is why we use the off-ice elevation differences to estimate the uncertainty of individual ASTER DEMs. To determine the correlation length (figure 4, revised manuscript) we use off-ice dh/dt calculated from elevations filtered using a symmetric cutoff (+10/-10).

Uncertainties.
Currently the uncertainties (on dh/dt but also on the region-wide mass budget) are purely statistical. It is just the goodness of the fit to the elevation time series. The authors end up with small error bars (less than 10%). It is not acceptable to provide such small error bars and then, in the discussion, make some sensitivity test to other sources of errors without incorporating them in the total mass loss uncertainties. I cannot accept statements such as "Changing the positive deviation allowed from +5 to +10 m yr^{-1} decreases the mass loss rate from 3.9 Gt yr^{-1} to 1.8 Gt yr^{-1} [*Note of the reviewer: more than a factor of 2!*]. The rate produced by allowing +10 m yr^{-1} is given as a rough minimum estimate of the mass loss rate". Other glaciologists need to know the real error bar on your mass loss assessment. Note that this remark also applies to your NPI and SPI studies. You can take advantage of this paper to revise those error bars.

We agree that these different values should be explained clearly from the start and will put them in the abstract. But in all of our papers, we consider a +10 m/yr deviation from first elevation to be much less likely than +5 m/yr (which we already regard as an upper bound). Therefore, we present it as an extreme lower bound on the mass loss rate that could be obtained from the ASTER and SRTM DEMs, and not as part of our ± uncertainty on the mass loss rate. To illustrate this point, consider the following scenarios that might result from adjusting the maximum allowed deviation. We find that if we allow +5 m/yr deviation from first elevation, we obtain a mass change rate of -3.9 Gt/yr for the CDI. If we allow a +10 m/yr deviation from first elevation, we obtain a mass change rate of -1.8 Gt/yr. Were we to obtain a mass change rate of, say, +1 Gt/yr allowing +30 m/yr deviation from first elevation, or +5 Gt/yr allowing +60 m/yr, we would not say our mass change rate is -3.9±8.9 Gt/yr. +5, +10, +30 and +60 m/yr deviations from the first elevation are not equally likely.

We recognize that it is difficult to assess the uncertainties when many elevations within certain DEMs are influenced by clouds and DEM errors. Our maximum allowed positive deviation of +5 m/yr already represents what we believe to be a reasonable upper limit (see above). +10 m/yr is double that. Hence, a mass change rate that is a factor of 2 less negative. Changing the cutoff from +5 m/yr to +10 m/yr will inevitably lead to the inclusion of erroneously high elevations (due to clouds) into our regressions over large portions of the icefield (and more than one bad elevation for many map points). As mentioned above, we will make it clearer in our text that we use an elevation cutoff, rather than excluding dh/dt after they are calculated.

Source of uncertainties that are not (and need to be) included in your region-wide mass balance are:

Value for the mean density of the material gain or loss. See example of various treatments of this question in (Gardner et al., 2012; Kääb et al., 2012; Zemp et al., 2010) among other papers.

A new section has been added to the manuscript to discuss the impact of different density assumptions on our mass change rate (section 4.1.3, "Impact of Density").

Gardner et al. (2012): 900±25 kg m^{-3}. This uncertainty applies to the entire volume change estimate and gives a final mass change rate of -3.9±0.5 Gt/yr (adding 0.1 Gt/yr to the uncertainty). Putting an uncertainty of this form on the density will yield a corresponding increase in the uncertainty of the final measurement proportional to the density uncertainty.

Kääb et al. (2012): 900 kg m^{-3} in ablation zone, 600 kg m^{-3} in accumulation zone: The final mass change rate using this approach is -3.6±0.4 Gt/yr.

Zemp et al. (2012): 860±60 kg m^{-3}. Applied the same way as for Gardner et al. (2012), gives a mass change rate of -3.7±0.5 Gt/yr.

Asymmetric threshold to filter outliers in dh/dt (see my proposition above).

Asymmetric threshold to filer outliers in dh/dt – see response above, we already use an asymmetric filter to remove erroneous elevations from the regression analysis and consider our method less arbitrary and less influenced by erroneous elevations than an n-sigma filter.

Uncertainties on the 2-m penetration of the C-Band SRTM DEM

Uncertainty due to the 2-m penetration of the C-Band SRTM DEM – this is addressed in the text, see section 2.2, tenth paragraph: "Penetration of C-band radar into ice and (particularly) snow (e.g., Rignot et al., 2001) is a potential problem when using the SRTM DEM to estimate dh/dt . Using a technique pioneered by Gardelle et al. (2012), Willis et al. (2012b) compare X-band SRTM elevations (which should have negligible penetration) with C-band SRTM elevations and find approximately 2 m of C-band penetration over the SPI at all elevations. Due to a lack of X-band SRTM coverage over the CDI a similar analysis fails to provide any insight here, however, we perform our processing with 2 m added to every SRTM elevation, which increases our mass loss rate by about 13%. The CDI is colder than the SPI, which could lead to drier conditions and greater penetration (Rignot et al., 2001), however, as noted above, we do not have adequate X-band data to quantify the difference. This effect should be considered when discussing mass loss from the CDI until such time as future studies help resolve the issue of radar penetration into ice and snow, which varies considerably depending on local conditions (Gardelle et al., 2012)."

Uncertain due to ELA

loss rate of -3.5±0.4 Gt yr^{-1}, a rate reduction of only 10%. This is expected as mass loss is concentrated well below the ELA."

2/ Improve the discussion to put the results of your 3 papers in a global perspective.
The 10-yr region-wide mass balances reported for the three large Patagonian icefields are among the most negative estimates. You could put these values in a more global context by comparing to other glaciers/icefields in the South Hemisphere / North Hemisphere. I think that the only other ice masses that are experiencing as negative mass balances as Patagonian glaciers are all in maritime environment: Icelandic ice caps (Björnsson and Pálsson, 2008) and also glaciers from the Yakutat icefield (see http://glaciers.gi.alaska.edu/events/igs2012/posters/63A438 but I did not find a published paper). Comparison to other glacier changes along the same latitude belt in the southern hemisphere would also be a useful addition (see specific comments with references below).
3/Improve the quantification and thus the discussion of the North/south asymmetry in mass balance for the CDI. Did (Lopez et al., 2010) also found similarly the N/S contrast that you suggest? More generally, I think it would be a strong addition to your paper to compare for individual glacier the length change to the mass balance. You could then discuss the usefulness of length change measurement (such as those reported since 1945 by (Lopez et al., 2010)) as an indicator of glacier health. Technically, it is relatively straightforward for you to find the length change between ca. 2000 and ca. 2010 and thus you would contribute to an interesting topic in glaciology. For example, (Arendt et al., 2002) found that: "*It is sometimes assumed that such changes in glacier length and area can be used to infer changes in glacier mass balance and response to climate, with retreat indicating an overall loss in glacier volume. However, we have found that during both the early and recent periods, about 10% of the sampled glaciers either advanced while simultaneously thinning or (during the early period) retreated while thickening (table S1). Even for those glaciers with the more "normal" response of retreat while thinning, we found a very low correlation between the rate of length change and the rate of thickness change*."

An estimate of the "North/South asymmetry" has been added to the manuscript (see response below for figures). Figure 1 shows the divide we use as a green line. We agree that monitoring glacier length changes would be interesting and relevant, and could be compared with previous results. However, while we may do this in the future, it is beyond the current scope of this paper.

Added, abstract: "Splitting the CDI along the main, east-west oriented highest divide results in a northern/eastern part with an average thinning rate of -1.8±0.2 m w.e. yr^{-1} and a southern/western part with an average thinning rate of -1.0±0.2 m w.e. yr^{-1}."

Added, introduction, first paragraph: "Throughout the paper the "southern" part or side of the CDI refers to southern and western glaciers, and the "northern" part or side of the CDI refers to northern and eastern glaciers. The green line in figure 1 shows the divide we use to distinguish between the "north" (1475 km^2) and ``south'' (1130 km^2)."

Added, section 3.1, second paragraph: "Splitting the CDI roughly along the main, east-west oriented highest-altitude divide produces an average thinning rate of -1.0±0.2 m w.e. yr^{-1} for the southern side, significantly lower than the northern side (-1.8±0.2 m w.e. yr^{-1}). The contrast between north and south is most likely due to an increased orographic effect (Holmlund and Fuenzalida, 1995)."

The figure numbering and arrangement has been changed to reflect the order they appear in the text.

4/ Dates. I am not found of MM/DD/YYYY (preferring DD/MM/YYYY which I think is recognized as the international format http://en.wikipedia.org/wiki/ISO_8601). I do not know if *The Cryosphere* as a standard format for dates (editors?) but at least you should defined clearly your convention to avoid confusion.

The manuscript has been changed to use DD/MM/YYYY.

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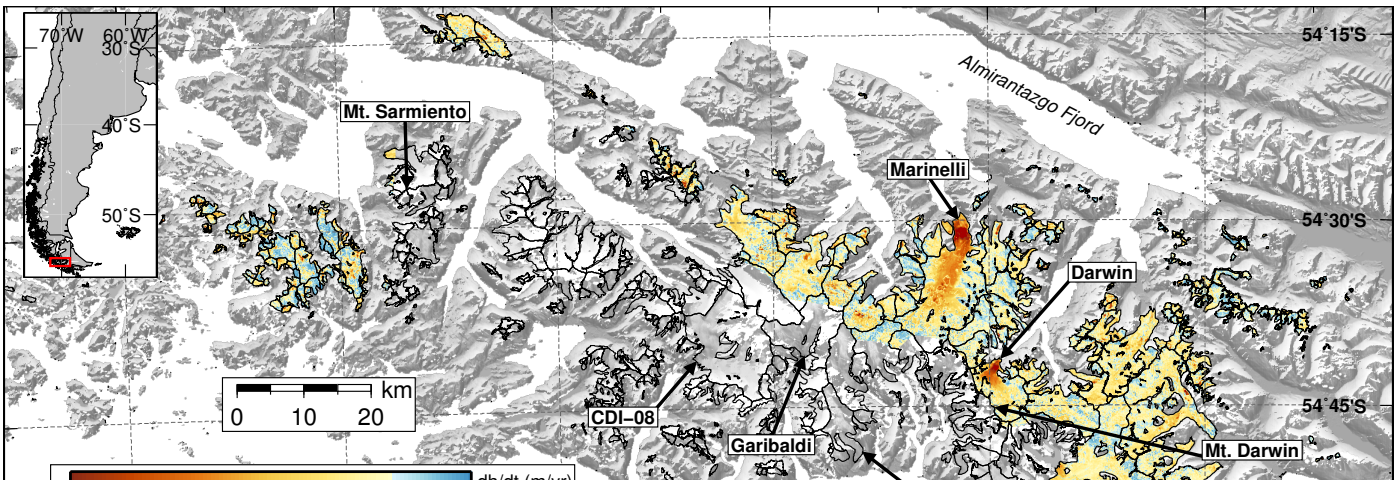
SPECIFIC and TECHNICAL COMMENTS
P3504 L09. I agree that it is important to sum up all region-wide mass losses to have a better estimate of the total glacier contribution to SLR but authors end up reporting mass balance with this unit only (and thus with lot of 0…). Here, rather than giving SLE, provide the area-average mass budget in unit of m w.e. yr^{-1} (or kg m^{-2} yr^{-1}) so that the imbalance from this region can easily be compared to others regions.

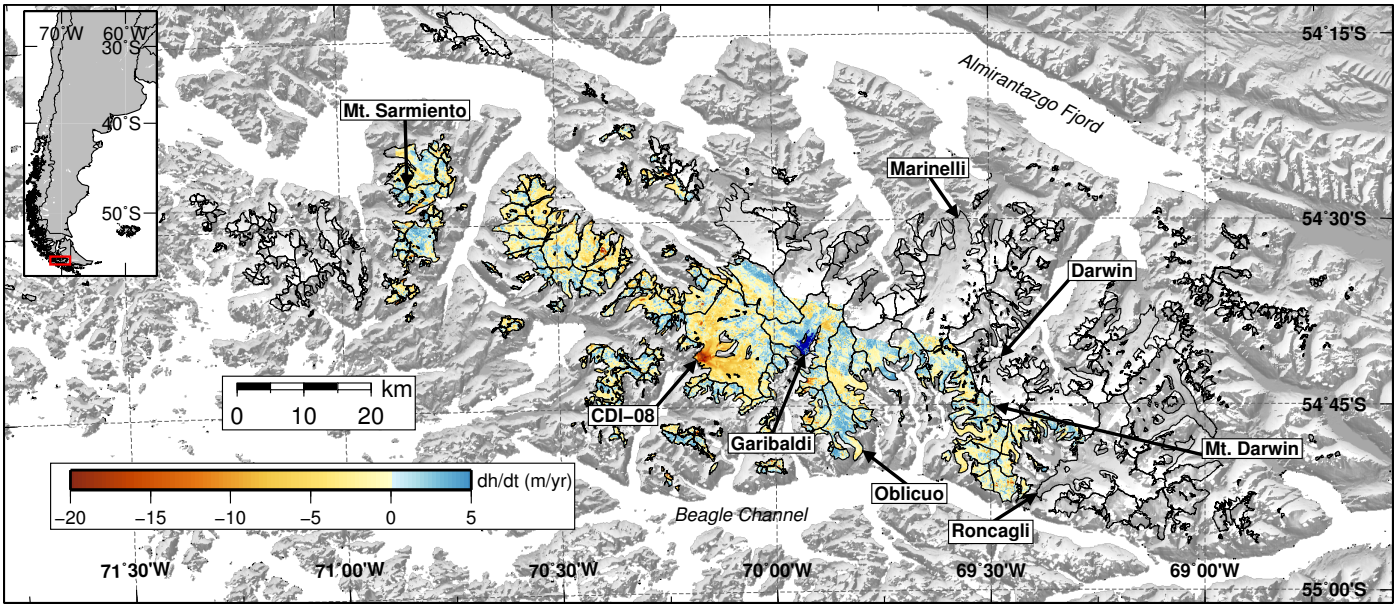
The manuscript has been changed to report this total in both mm yr^{-1} and m w.e. yr^{-1}.

P3504 L11 "Thickening is apparent in the south". What do you mean by apparent? Is the thinning stronger in the north or in the south? I suggest that you split the CDI into his North/South side and compare the mass balance.

Splitting the CDI along the main, east-west oriented, highest divide yielded a "northern" (northern/western) part of the icefield (1475 km^2) with an average thinning rate of 1.8±0.2 m w.e. yr^{-1} and a "southern" (southern/western) part (1130 km^2) with an average thinning rate of 1.0±0.2 m w.e, yr^{-1}. Garibaldi Glacier in particular shows a strong thickening signal, which is consistent with its advance (figure 3). Aspect was also considered in defining the northern and southern zones.

"Northern" side of CDI





See previous response for additions to manuscript.

P3504 L20. According to Figure 1, it seems that you measured more than just a single icefield but that they are also many ice bodies not connected to the main icefield. Clarify.

Our study region includes the 2300 km² of the CDI (Lopez et al., 2010), plus about 300 km² of peripheral glaciers, which together with the CDI are the remnants of a much larger Tierra del Fuego ice body (e.g. Glasser et al., 2008). Figure 4 of Lopez et al. (2010) demonstrates that previous definitions of "the CDI" include non-contiguous ice bodies. All areas in figure 1 are considered as part of the CDI.

P3505 L09. (Berthier et al., 2007) does not deal with Alaska or Patagonia and is not a relevant reference. Other southern ice bodies have been measured at about the same latitude and could be referenced here (probably more relevant than studies dealing with Alaskan icefields, far away in the other hemisphere). Later in the discussion of the paper, their mass balance could also be compared to yours. It seems to me that those and your studies in Patagonia contribute to map a strongly negative mass balance (more negative than 1 m w.e./yr thus about 2 to 3 times more negative than the global average of glaciers) of most glaciers along a latitude belt at the 40°S – 50°S. References (maybe more?) Heard Islands (Thost and Truffer, 2008), Kerguelen Island (Berthier et al., 2009) and South Georgia (Gordon et al., 2008)

The Berthier reference has been removed. Thank you for the new references, these are added to the manuscript and their results are provided alongside our own to give a regional context.

Added section 4.1.5, "Comparison with other Southern Hemisphere Glaciers"

Added, conclusions, fifth paragraph: "Other glaciers at the same latitude (e.g. the Kerguelen Islands, the island of South Georgia, and Heard Island) are thinning and retreating, and have

assessment.

The citation has been added to the manuscript.

Added/changed, introduction, fourth paragraph: “Mass loss at the CDI might be contaminating GRACE measurements of the Antarctic Peninsula, NPI and SPI (Ivins et al., 2011), so our constraints on the mass loss rate occurring at the CDI will help isolate this signal.”

P3506 L13. Rather than comparing SLR contribution from different icefields (largely controlled by their total area), I recommend that you compare their mass balance.

The conclusion has been changed to give area-averaged dh/dt in m w.e. yr^{-1}. The area-average dh/dt for the CDI is -1.5±0.2 m w.e. yr^{-1}.

P3506 L18. Not clear here why you are interested in deriving mass fluxes (although we understand later why).

Text has been added here to help illustrate the importance of mass flux.

Added, introduction, fifth paragraph: “A high mass flux through the front of the glacier can contribute to thinning if it is greater than the input mass flux. This would be "dynamic thinning", or thinning due to ice motion.”

We have also revised the discussion and conclusion to more clearly convey our purpose in estimating the mass flux.

P3507 L05. As said in my general comments, I did not find in the previous references some convincing arguments that the Landsat GLS and the SRTM DEM are well co-registered. Need to be improved.

Again, see the Tucker et al. (2004) reference. The important point here is how well the ASTER DEMs end up being co-registered to the SRTM DEM. We provide a good measure of this as our uncertainty for each individual ASTER DEM is from the off-ice elevation differences between the ASTER DEM and the SRTM DEM.

P3507 L26-28. I would move this sentence, relevant for the ablation area, just after “(Fig. 10)” (=one sentence up)

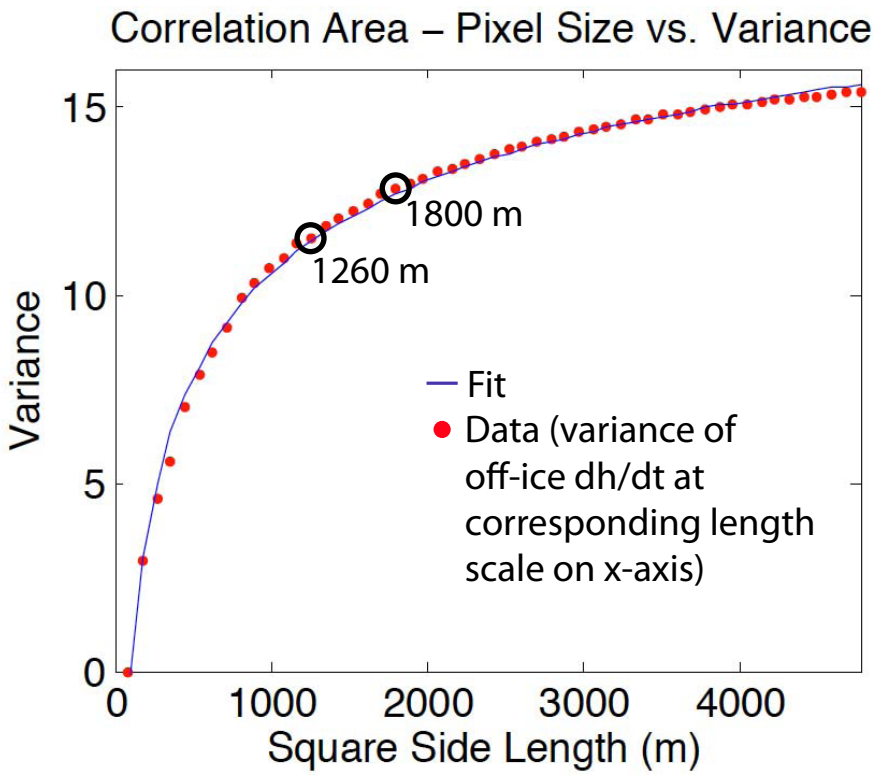
The sentence has been moved to improve the flow of the text.

P3509 L02. “begins to “flatten” ” is not precise enough. How did you exactly determine the decorrelation length? My question is further justified by the fact that although you used the same dataset (SRTM and ASTER DEMs) in your three studies, the decorrelation length in your off glaciers

details on the method), which we estimate to be 1800 m by 1800 m (figure 4). This is analogous to the "corner" point on an L-curve (e.g., Aster et al., 2005, pg. 91, figure 5.2) and indicates the lengthscale past which the dh/dt are no longer correlated."

The selection of the decorrelation length is analogous to the selection of the corner point in an L-curve when performing an inversion – the point closest to the "corner" is chosen (e.g. Aster et al., 2005, pg. 91, fig. 5.2). There is some subjectivity inherent in the selection of this corner point, especially when the corner is not distinct (see figure below). A 2nd-order polynomial fit has been made to log(x)/y, choosing the point of maximum curvature would actually result in a much lower uncertainty, instead we want the point where the curve begins to "flatten", which is analogous to choosing the corner point of an L-curve. Choosing 90 m or 4770 m as the "corner" point or the point that the curve begins to "flatten" would be wrong, there is a limited range of points to select from and we chose 1260 m. Given that there is range of possible "corner" points, we have taken 1800 as more "conservative" estimate of the decorrelation length. All uncertainties have been adjusted accordingly. Willis et al. (2012b) on dh/dt at the SPI includes estimates of thinning at the NPI that take 1800 m to be the decorrelation length. However, Rolstad et al. (2009) note that the decorrelation length is not expected to be the same at each icefield as it is dependent on the particular characteristics of the landscape.

Figure 4 of the revised manuscript has been modified to more clearly illustrate these points. The figure below includes both our previous corner point and our new corner point:



P3509 L29. Reference to the original paper proposing to compare band X and C (Gardelle et al., 2012) is needed here. Currently, it read as if (Willis et al., 2012b) were the first to use this method to provide a first order estimate of the C-Band penetration.

reason Willis et al. (2012b) is given as a reference here is because the sentence is specifically referring to the SPI, which is the closest Patagonian icefield for which X-band data is available and this analysis performed.

P3510 L19. Given that you obviously cannot cite all papers mapping glacier velocity (and thus your selection will be necessarily arbitrary), I would only cite the initial work by (Scambos et al., 1992).

Changed, section 2.3.1, first paragraph: "This technique, known as "pixel-tracking", has been used to track velocities on many glaciers (e.g., Scambos et al., 1992)."

P3510 L26. Citing a Ms Thesis is not really useful especially if you do not provide the URL. Does it contain any relevant information that is not yet in the (Willis et al., 2012a) NPI paper?

Citation of the thesis has been removed from the manuscript.

P3511 L02. (Nuth and Kääb, 2011) proposed a linear elevation dependant correction but applied to a map of elevation difference not a velocity field. I did not really manage to figure out what is exactly the origin of the elevation dependent bias on the displacement map, which to my knowledge, has not been reported previously (reading (Scherler et al., 2008) may provide some insights into the origin of this error).

Ahn and Howat (2011) reported this problem (section IVA): "The surfaces of large glaciers and ice sheets tend to be of low relief, with typical surface slopes on the order of 0.01, so that such an error is nearly systematic over the ice surface. Therefore, the error in displacement resulting from co-registration error can be substantially reduced by subtracting the displacement of stationary (off-ice) control points located close to the same elevation as the glacier surface. These control points may be selected manually, or the process may be automated by specifying an off-ice mask." The latter is the method we use.

P3511 L15. Any references to a previous work measuring glacier surface velocities using tracking of ALOS L-Band data? The reference that pops up in my mind is (Strozzi et al., 2008) but they did not use ALOS but JERS (also L-Band SAR though). Maybe you will find another one?

The following references have been added to the manuscript:

Rignot, E., "Changes in West Antarctic ice stream dynamics observed with ALOS PALSAR data ", Geophysical Research Letters, 35, doi:10.1029/2008GL033365, 2008

Strozzi, T., Kouraev, A., Wiesmann, A., Wegmüller, U., Sharov, A., and Werner, C., "Estimation of Arctic glacier motion with satellite L-band SAR data", Remote Sensing of Environment, 112, 636-645, 10.1016/j.rse.2007.06.007, 2008

Rignot, E., Mouginot, J., and Scheuchl, B., "Ice Flow of the Antarctic Ice Sheet, Science", 333, 1427-

important given the rapid thinning of those ice masses.

The 2011 Quickbird imagery is orthorectified to a 2007 ASTER DEM, the 07/09/2001-25/09/2001 and 06/09/2003-13/09/2003 ASTER image pairs are now orthorectified to the 25/09/2001 and 13/09/2003 ASTER DEMs, respectively. This improved the direction of the velocity vectors for slower areas (especially for the 2003 pair). All figures and numbers have been adjusted to use the new results, though the front speeds and the uncertainties did not change for these two pairs. The ASTER image pair over Darwin (25/09/2001 to 02/10/2001) remains orthorectified to the SRTM, as the direction of the velocity vectors already appeared reasonable and using the SRTM to orthorectify imagery for pixel-tracking is an accepted technique (e.g. Scherler et al., 2008). All other ASTER pairs are orthorectified to the SRTM DEM.

Added, section 2.1: "All ASTER images used for pixel-tracking are orthorectified to the SRTM DEM except for the 07/09/2001 to 25/09/2001 and 06/09/2003 to 13/09/2003 pairs covering the front of Marinelli Glacier (due to the pronounced thinning there). These pairs are orthorectified to the 25/09/2001 and 13/09/2003 ASTER DEMs, respectively."

Changed, section 2.3.4, second paragraph: "The two ASTER pairs for which we obtain front speeds at Marinelli are from 2001 (07/09/2001-25/09/2001) and 2003 (06/09/2003-13/09/2003). We coregister the earlier image in each of these pairs to the later image, and orthorectify using the DEM of the later image."

P3512 L15. The fact that thinning can influence the velocity measurement is not as simple as described here. It depends on the incidence angles during the image acquisition. If both images are acquired at Nadir (case of Landsat and some ASTER images) or with the same incidence angle (=low B/H), there is no sensitivity to an error in the DEM or an elevation change between the date of the DEM and the dates of the image pairs. Examining the incidence angle of the image pair (B/H parameter) + the dh/dt map you could provide an estimate of the error due to this effect. More subtle but although a possible source of error that you could mention is the one that is due to thinning that occurs between the two image of a pair (you measured as much as 2 m/month of thinning at some locations).

The incidence angle for the 07/30/2011 QB02 scene is 19.4 degrees, for the 08/16/2011 QB02 scene it is 19.2 degrees, so there should be little sensitivity to DEM error/elevation change, $B/H=\tan(\alpha)$ (e.g., Fujisada et al., 2005) so the B/H for this pair is 0.003. The two ASTER pairs covering the front of Marinelli (2001 and 2003) have been reprocessed so that the earlier scene in each pair is coregistered to the later scene and orthorectified using the DEM of the later scene (see above response). This eliminates widespread error due to thinning between the DEM time and the image time (Scherler et al., 2008) given that the DEM is now coincident with the imagery.
The new results show improvement to the direction of some of the velocity vectors.
These changes have been reflected in section 2.3.4 of the text and figures 9 and 10 (revised manuscript) have been updated to use the new velocities/speeds. Uncertainties were recalculated but remained the same at the level of significance used in the paper. Existing speeds did not change significantly (i.e. maximum speeds remain the same) but we were able to fill in some areas, and the vectors indicating direction appear to better reflect what we would expect.

difference in incidence angle between the two QuickBird 2 images is less than 0.2 degrees. This means the base/height (B/H) ratio (e.g., Fujisada et al., 2005) is low (0.003) and the pair is insensitive to DEM errors or elevation change between the time of the DEM and the time of image acquisition.

The two ASTER pairs for which we obtain front speeds at Marinelli are from 2001 (07/09/2001-25/09/2001) and 2003 (06/09/2003-13/09/2003). We coregister the earlier image in each of these pairs to the later image, and orthorectify using the DEM of the later image. Orthorectifying the ASTER imagery to the coincident ASTER DEM minimizes disparity due to DEM errors (with error due to thinning almost entirely removed) and difference in incidence angle between the ASTER images (e.g., Scherler et al., 2008, equation 1).

One ASTER image pair from 2001 (25/09/2001 to 02/10/2001) contains front speeds for Darwin Glacier, we use the results obtained by orthorectifying to the SRTM DEM because the direction of the velocity vectors already appeared consistent with the flowlines in the ASTER imagery of Darwin Glacier. Velocities over CDI-08 are from radar pixeltracking so orthorectification errors do not apply because radar images are not orthorectified."

P3513 L2. "Significant". Statistical sense? If not, I would simply start the sentence with "Marinelli, Darwin and CDI-08 glaciers account for…". What is the percent of the CDI area covered by these three glaciers? In others words do they have cover significantly less than 31% to justify reporting their mass loss separately?

"Significant thinning" refers to dh/dt that are significantly more negative than those estimated for other glaciers (e.g. -20 to -25 m/yr versus 0 to -10 m/yr). This thinning is statistically significant, both in the sense that it is higher thinning than other glaciers (and the difference is statistically significant) and in the sense that the combined volume loss rate of these three glaciers is larger than the uncertainty (see the uncertainty attached to the volume loss estimate on line 3 of the same page). This sentence regarding the volume loss rate of these three glaciers anticipates the reviewer's suggestion "for the three glaciers with large losses those values can be given in the main text" made later in the review. From table 1, these glaciers constitute ~12% of the total icefield area (using the total icefield area given in the introduction).

Changed, section 3.1, first paragraph: "31% of the mass loss occurs at just three glaciers (Marinelli Glacier, Darwin Glacier, and CDI-08 Glacier) which cover 12% of the icefield area."

P3513 L4. Here it would probably be the good place to compute/compare area-average thinning for the northern and southern part of the icefield.

Per previous response, splitting the CDI along the main, east-west oriented, highest divide yielded a "northern" (northern/eastern) part of the icefield with an average thinning rate of 1.8±0.2 m w.e. yr^{-1} and a "southern" (southern/western) part with an average thinning rate of 1.0±0.2 m w.e, yr^{-1}.

P3513 L10. Why "successfully"? One can always track pixels on whatever images.

Changed, section 3.2, first paragraph: "Pixel-tracking provides useful velocities…".

Reference to the congress has been removed.

Changed, section 4.1.2, third paragraph: "In-situ measurements of accumulation rate on the CDI are required to refine our estimates further; the cutoffs we use are the best available."

P3517 L3. The "anomalous" behavior of this glacier would be better illustrated if we knew the mean mass balance of all southern side glaciers.

The southern side of the CDI has an average thinning rate of -1.0±0.2 m w.e. yr^{-1} (see previous comments). CDI-08 has an average thinning rate of -3.0±0.5 m w.e. yr^{-1}. Oblicuo and Garibaldi are examples of two more "typical" southern glaciers, they both have positive dV/dt (and Garibaldi is known to be advancing, see figure 3, revised manuscript). Contrasting CDI-08 with these glaciers by examining to figure 1, figure 3 (revised manuscript) and table 1 (which shows that Garibaldi and Oblicuo have positive dV/dt, albeit with high uncertainty) highlights the "anomalous" behavior of CDI-08.

Added, section 4.1.4, fourth paragraph: "The average thinning rate for CDI-08 is -3.0±0.5 m w.e. yr^{-1}, compared to -1.0±0.2 m w.e. yr^{-1} for the southern part of the CDI."

P3517 L15. Any good reason to suspect a "surge-like" behavior on this glacier? I never heard of surging glaciers in Southern Patagonia but may have overlooked the papers presenting them.

The following reference has been added to the manuscript:

Rivera A., Aravena J., Casassa G., "Recent fluctuations of Glaciar Pío XI, Patagonia: discussion of a glacial surge hypothesis", Mountain Research and Development, 17(4), 309-322, 1997

Changed, section 4.1.4, paragraph 5: "Our results indicate the possibility of surge-like behavior (e.g. Rivera et al., 1997), …"

P3517 L22. This statement (steepening of the glacier -> acceleration) makes sense but without any numerical verification it is not really useful. Indeed thinning also means that the thickness is decreasing and at first sight one cannot guess which of decreasing thinning or increasing slope has the largest influence on the driving stress (and thus the velocity).

The 2011 front is moving at speeds approaching 10 m/day, and this is an area were speeds were previously 5-6 m/day. The equation for driving stress is $\sigma=\rho gh*\sin(\alpha)$, where ρ is density, g is acceleration due to gravity, h is thickness, and α is surface slope. Is the thickness changing? Yes, it is decreasing, which would decrease the driving stress, whereas we see speeds increasing. However, the slope here has steepened, so α is increasing. The sine function increases as the angle increases from 0-90 degrees (the range we are operating in), so an increasing α will increase the driving stress. This can be concluded without numerical verification. If speeds were decreasing then it would likely mean that decreasing thickness was influencing driving stress more than increasing slope. It is not useless to state that slope is increasing and that this would contribute positively to driving stress, this is

that “small increases in slope can result in significant increases in terminus velocity, allowing for rapid surges and appreciable increases in calving.” They also state that thinning associated with slope increase actually has the potential to “enhance calving velocities”, presumably by reducing basal drag.

P3517-18. I had some difficulty to follow this part of the discussion and in particular the comparison to the work by Koppes et al. An effort to re-organize your discussion and explain more clearly what Koppes et al. did would help the reader to position your work compare to them.

This section has been split up into several different parts to more clearly convey the message we are trying to deliver in each subsection. Text has been cut from the flux discussion and Koppes et al. (2009) comparison, and we spend more space discussing what our combined velocity and dh/dt observations tell us about Marinelli Glacier. An overview has been added to more clearly explain the discussion of Marinelli Glacier velocities/speeds and what we conclude from them.

Added, section 4.2.1, “Marinelli Glacier – Overview”: “Below, we estimate flux for Marinelli Glacier using our speeds, then compare our results with Koppes et al., 2009, who estimate the terminus speed and flux of Marinelli from the retreat rate. We find that our results do not agree with Koppes et al., 2009. Whereas they infer a reduction in terminus speed for Marinelli from 2000 to 2005, we find that the front speed in 2003 and 2011 is at least as high as 2001, and consequently the flux in 2011 is approximately the same as the flux in 2001. We conclude that thinning at Marinelli Glacier is probably dynamic, with bed geometry likely governing velocity and retreat. We then consider Marinelli Glacier as a tidewater-cycle glacier (TWG) in retreat phase (e.g., Meier and Post, 1987; Motyka et al., 2003; Post et al., 2011), and compare it with Jorge Montt Glacier on the SPI.”

Added/changed, section 4.2.2, “Marinelli Glacier – Flux”
Added/changed, section 4.2.3, “Marinelli Glacier – Comparison with Previous Results”
Added/changed, section 4.2.4, “Marinelli Glacier – Thinning Gradient Maintains Surface Slope at the Front”
Added/changed, section 4.2.5, “Marinelli Glacier – Tidewater Cycle”

P3518 L10. Why do you assume the height of the front wall? You should be able to measure it from the DEMs?

Fluxes are now estimated by measuring the glacier height from the nearest DEM (in time) and adjusting based on dh/dt if necessary.

Added/changed, section 4.2.2 – “Marinelli Glacier – Flux”: “We calculate flux along transects perpendicular to glacier flow (as close as possible to the front) for velocities from 07/09/2001 to 25/09/2001, 06/09/2003 to 13/09/2003 and 30/07/2011 to 16/08/2011. The height of the glacier is determined from the 25/09/2001 ASTER DEM, 13/09/2003 ASTER DEM, and the 13/11/2007 ASTER DEM (adjusted using our dh/dt) respectively, we assume an average glacier depth below water of 150 m (see Koppes et al., 2009, figures 4a and 4b). Adding this to the height gives an approximate thickness. We multiply the glacier thickness by the perpendicular velocity along the transect to calculate flux. Sources of uncertainty that we include in the uncertainty for our flux estimates are the uncertainties on the speed, uncertainty on the depth below water (±50 m), and

…”

P3519. Regarding tidewater glacier dynamics, I think earlier work by (Meier and Post, 1987) or more recent papers by (Motyka et al., 2003) could be referenced.

These references have been added to the manuscript.

P3519 L16. ‘.’ After September.

Corrected.

P3519 L22. You should tell us why “you consider unlikely”. Seasonal variations of this amount (or much more) have been observed on tidewater glaciers elsewhere. Just one example among others (Kääb et al., 2006).

Seasonal variations of this amount or more have been observed on tidewater glaciers elsewhere, but we do not have the data to say whether the acceleration has been sustained. We consider it plausible that the rate of motion is linked to the long term thinning observed at the front, as the glacier adjusts to a new geometry.

P3520. We do not learn much from 4.2.2…

This is section 4.2.6 in the revised manuscript. It refers to other glaciers on the CDI. We believe it is useful to have estimates of thinning and baseline speeds for these glaciers. The text reinforces the information conveyed by the figures (e.g. figure 9, revised manuscript, shows Marinelli, Darwin and CDI-08 speeding up towards their fronts, but Roncagli slowing down).

P3520. Part of the conclusion just repeats the introduction and part of it should be in the discussion. Your conclusion could be improved.

The conclusion has been revised to better convey the primary findings and interpretations of the study.

P3521 L2. “goes” -> “went”

Corrected.

P3521 L13. Comparison to the Juneau icefield is totally off topic. Why this icefield and not others? Furthermore, the work cited is not peer-reviewed. As stated earlier comparison to other ice masses (i) with similarly rapid mass loss or (ii) in the Southern hemisphere would make more sense.

Table 1: provide also (only?) the glacier-wide mass balance for each glacier so that the reader can compare them. I do not find the individual glacier mass loss useful (for the three glaciers with large losses those values can be given in the main text). For relative contribution of the accumulation/ablation area, what about giving just their relative % instead? Three final rows with the sum (or area-weighted mean) of values for the North only / South only / All glaciers would be a useful addition. An additional column with length change measurements?

Table 1 has been modified to give glacier name, area, dV/dt (with uncertainties) and average w.e dh/dt (with uncertainties). The columns for accumulation and ablation zones have been removed. Three rows have been added to the end to include "northern", "southern" and entire icefield totals. We are not going to do length-change measurements in this paper, though we agree they are a useful metric. Length changes have been previously recorded for the CDI by Holmlund and Fuenzalida (1995) and Lopez et al. (2010).

Figures. I am puzzled by the order of the figures…. Did a random program generate this order?

The numbering of the figures has been changed to reflect the order they are referred to in the text.

Figure 1. North of Garibaldi and CDI-08 glacier, one glacier is showing an unexpected pattern of the dh/dt (seems typical of a North/south shift in the compared DEM). To double check.

We compare the hillshade SRTM and corresponding Landsat image to the hillshade ASTER DEM and corresponding ASTER V3N image for two ASTER DEMs that cover this region (02/24/2008 and 01/15/2011) and are incorporated into the regression (we note here that we perform a regression rather than a difference, so there is not one "compared DEM" but rather several DEMs). There does not appear to be mis-coregistration between the ASTER V3N and Landsat or mis-coregistration between the SRTM DEM (at 90 m resolution) and the ASTER DEM hillshade. The ASTER imagery reveals what is likely the real culprit – snow and cloud on the south-facing slope of Broken Glacier, which is consistent with weather patterns in this region (e.g. Holmlund and Fuenzalida, 1995). The cloud is largely excluded by our cutoff (it appears in the 2011 DEM, but a profile of included elevations shows that most of the cloud-covered areas from this DEM are excluded). The snow undoubtedly has some influence on the DEM, though the signal might be real.

Figure 4. Indicate the location of the calving front in 2011 also.

The location of the 2011 calving front has been added to figure 10 (revised manuscript) as a yellow line.

Figure 5. remove the quote before (c) in the legend.

Removed (figure 11, revised manuscript).

Figure 8. Not really useful. What do you mean by "These are from a total of 1 QB02 pair, 3 ALOS pairs and 119 ASTER pairs processed"? Why those pairs were excluded? Clouds? Low correlation?

Figure 7 (revised manuscript) shows the time intervals covered by our velocity results. Removed "These are from a total of 1 QB02 pair, 3 ALOS pairs and 119 ASTER pairs processed". The results from those pairs were excluded because they did not contain discernable glacier motions. This was due mostly to clouds, which lead to poor pixel-tracking results (indicated by a low signal-to-noise ratio, i.e. the peak correlation coefficient is similar to the average correlation coefficient due to uniformity within the search area). In some cases it was because overlap was over snow, which also leads to poor pixel-tracking results.

Figure 9. Legend could be improved so that we really understand what is plot here and what the figure really shows.

Figure 4 (revised manuscript) has been updated to more clearly convey that it is the off-ice dh/dt variance at different length scales, and that the point where the curve begins to "flatten" (i.e. the variance stops changing significantly) is the point at which the dh/dt are no longer correlated (i.e. the "decorrelation length").

Figure 10. It would be best if the # followed a logical order (1-10 from low to high elevations). Nice figure. Can you add the value for the slope of the trend (=dh/dt) on each panel?

These changes have been applied to figure 2 (revised manuscript).

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