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.

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.

(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.

(ii) If for whatever reasons (i) does not work or does not convince you (?), I propose that you include in your uncertainty an estimate of the bias induced by using an asymmetric range of "acceptable" 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.

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.

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.

Asymmetric threshold to filter outliers in dh/dt (see my proposition above).
Uncertainties on the 2-m penetration of the C-Band SRTM DEM
Uncertain due to 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.*"

3/ Figures do not appear in the order they are referred to 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.

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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.

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.

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.

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)

P3506 L08. Here, citing (e.g., Ivins et al., 2011) seems also relevant for a region-wide GRACE assessment.

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

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

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-coregistered. Need to be improved.

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

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 rate of elevation difference maps are varying from 1260 m (here), 720 m (NPI) and 1800 m in the SPI. The factor of 2-3 difference is not really expected...

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.

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).

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?

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).

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?

P3512 L05. It is not clear to me which images were orthorectified using SRTM? I though the ASTER images were orthorectified using the corresponding ASTER DEM (same date). Clarify. It is important given the rapid thinning of those ice masses.

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).

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?

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.

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

P3515 L25. The paragraph is a bit weak and I do not really see the point in referring to a congress to say that we need more reliable measurement in the accumulation areas... Such an obvious need. The last sentence of the paragraph is probably sufficient.

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

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

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).

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.

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

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.

P3519 L16. '.' After September.

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).

P3520. We do not learn much from 4.2.2…

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

P3521 L2. "goes" -> "went"

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?

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

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.

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

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

Figure 7. Maybe not enough referred to in the text (?).

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

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?

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