Review of Gardner et al., TCD, May 2012

SUMMARY

Gardner and co-authors take advantage of numerous recent (post-1995) surface elevation measurements (satellite DEMs, satellite and airborne laser altimetry) and compare them to older maps, mainly from the 1960s, to measure volume and mass changes of glaciers and ice caps on Baffin and Bylot islands (southern part of the Canadian Archipelago). Thus, they put the recent (2003-2009) mass losses for the same regions (Gardner and others, 2011) in a multi-decadal perspective and conclude convincingly to an accelerated mass loss since the mid-1990s. The increase in mass loss is related to the increase in regional air temperature with, interestingly, different temperature sensitivity for the two main ice caps (Barnes and Penny).

GENERAL COMMENTS

The paper is well written, the methodology is up-to-date and the results will be of interest to the glaciological community (and beyond). The paper deserves to be published in TC. The aim of my review is mainly to suggest some possible improvements/clarifications.

1. Improved discussion through comparison to the published literature.

In the submitted paper, there is no comparison to published mass loss for the same region within global estimates. (Dyurgerov and Meier, 2005, his region 22, Page 103) and (Cogley, 2009) / (Church and others, 2011) have published some long term mass balance estimates for the same region (probably, by necessity, based on sparse mass balance measurements or using some extrapolation from mass balance data even outside the study region). It could strengthen your discussion if you could compare to those previous estimates. You could also compare to (Hock and others, 2009) and maybe others global modelling effort (van de Wal?) that need to be confronted against regional assessment of mass changes (such as yours).

You did not really compare your results with (Sneed and others, 2008) who also previously reported an accelerated rate of mass loss for a single transect on Barnes Ice Cap. Here again you would strengthen your discussion.

You could also compare your **mass balance sensitivity** to temperature changes to previous estimates of this variable. In particular from (Hock and others, 2009) who can probably extract for you their parameter S^T for the Barnes and Penny Ice Caps. It is important to test/validate those sensitivities because they are used to project future mass losses from glaciers (e.g., Radic and Hock, 2011). Do the authors have an explanation to the different mass balance sensitivities to temperature of the Barnes and Penny Ice Caps (although I understand this is beyond the scope of the paper to discuss this in details).

I agree with Neil Glasser (his SC) that it is relevant to compare to their study (Glasser and others, 2011) that put into a longer term perspective the recent mass loss of the Patagonian Ice Fields. (Willis and others, 2012) may also be cited for recent ice losses in Patagonia. The same holds for Alaskan glaciers where acceleration of the mass losses has been reported (e.g., Arendt and others, 2002).

2. Drivers of the accelerated mass loss

Apart from the albedo feedback (already discussed in the paper), the elevation feedback is sometime also invoked to explain accelerated ice loss. Could the authors also provide a 1st order-estimate of this feedback? For Barnes Ice Cap, they can for example estimate the mean surface lowering of the ice cap between different time intervals and thus the corresponding rise in temperature due a lowering glacier surface (e.g., using a constant temperature lapse rate with altitude, maybe from reanalysis?).

3. Sampling from sparse altimetry

Because the authors have complete maps of elevation changes (at least for Barnes), they can more thoroughly examined how well sparse elevation measurements can be used to infer the total volume change. They could sample the map of the elevation change (SPOT5-CDED) where (i) they have ATM dh/dt and (ii) they have ICESat dh/dt and compare the total volume loss. They would thus only look at sampling effect without influence of the accuracy of the different measurements. In others words, when the mass loss for Barnes is compared -2.9 Gt/yr (SPOT5, 1960-2010) and -2.5 Gt/yr (ICESAT, 1960-2006), the differences are due (i) sampling but also (ii) different time stamp for the recent survey and (iii) altimetric difference between SPOT5 and ICESat so that it is not an unambiguous demonstration of the lack of sampling bias. Regarding (ii) it is stated in the paper that: "most of the difference is due to the sampling interval". Could you be more convincing? If this holds, you need at 4-year 2006-2010 mass loss of 7.5 Gt/yr (larger but not far from your 6.2 Gt/yr for 2005-2011)

4. Seasonal correction

The DEMs are stated as "generated from late summer imagery". 7 July is not exactly the end of the summer in the arctic… Cannot you use the GRACE time series (10-year mean seasonal cycle) to propose an error estimate due to the fact that all data are not acquired at the end of the melt season? I foresee a small error due to the long time separation but, for completeness, it would be nice to see this issue address in the paper. What about the 10 March 2010 DEM? Did you do any seasonal correction?

5. No Need for updating GRACE analysis

I do not see the point in updating GRACE estimate. It makes the paper much longer (two pages in the method are dedicated to GRACE) and the mass loss for 2003-2011 (-23.8 ± 3.1 Gt/yr) equal the -24 ± 7 Gt/yr value from (Gardner and others, 2011). By the way, it is not explained why the error bar is lowered by a factor of two. I strongly recommend shortening the paper by not updating the GRACE time's series.

SPECIFIC and TECHNICAL COMMENTS

P1564
Title. Probably too long and "long-term" is vague (multi-decadal?). Do you need to enumerate the different datasets?
L19. Why do you change unit for the recent and multi-decadal mass losses (mm and mm/yr)

P1565
L1. Give the exact area also (41000 km^2 is not exactly one third of 147000 km^2, rather 28% so closer to one fourth)
L5. Reference to (Sneed and others, 2008) here?
L16-17. This sentence is "method", not needed in the intro

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P1566
L12. “of the” repeated

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P1567
L3. “gradient” is unclear (maybe simply remove)

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P1568
L3. “:” needed in the title?
L18-20. How did you treat the regions where gaps in CDED are filled using modern satellite data? Could you identify those unambiguously and exclude them?
L23 and L26. Use same number of decimals for consistency

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P1569
L8. Not sure “detection” is the best world. “measurement” instead?
L15. This is the maximum achievable ground coverage. In general case the data strip is not as long as 600 km (the 120 km swath is constant)
L22. Berthier and others (2007) used a DEMs derived from SPOT5-HRG imagery not SPOT5-HRS so they cannot be cited about “Similar DEMs…”. A study where SPOT5-HRS DEMs were used in complex glaciated terrain: (Gardelle and others, 2012)
L23. Can you progress logically from North to South when you describe the DEMs?

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P1571
L11-12. This is on ice? (if this is on stable ground threshold on dt not needed)
L19-20. I did not understand

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P1572
L25. “cf.” not needed

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P1573
L10-14. This section is not really clear. In Fig A2, a slope dependent bias is mentioned first and then spatially dependent bias. So is a slope-dependent bias detected or not?

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P1574
L15. “from repeat measurements acquired five year apart” probably not needed
L26. Barnes (check everywhere)

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P1575
L13. The two sigma filter is dangerous. You can end up removing real measurements due to strong spatial variability within one elevation band. Did you check that it was not the case? This is especially the case if the filter is applied to all ice within a region. Or did you apply it to each glacier separately? More generally, did you test the sensitivity to this filter?

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P1578

L15-17. Could probably be explained a bit more clearly (or illustrated with a map for one ice cap?)
L28. Penny. Check everywhere

P1579
L11. Where does ±25 come from? 0.9 + 0.25 = 0.925 > density of pure ice
L17. space missing

P1580
L6. Could be clarified. For all ice area I would not have done the RSS sum but simply the sum of the individual uncertainties. Am I wrong?

P1581
L23. If ever GRACE longer time series was retained in the revised paper, could you provide at the end of the paragraph the total uncertainty and explain the difference with Gardner et al. 2011?
L25 (to L3). Unnecessary repetition.

P1582
L19. “from”

P1583
L8. “a strong pattern of low elevation ablation”. You do not measure ablation but elevation changes this is (really!) different.
L15-17. I do not think the explanation is needed for TC readership.
L26. “smaller” is always ambiguous for negative values (a solution would be to add “absolute” before elevation change)

P1584
L3. “Barnes”
L21-24. Why do you extrapolate from measurements on “Penny Ice Cap” only?

P1585
L14. Did you compare to Jacob et al for the exact same period? Disturbing nearly 50% difference between the two estimates…
L19. The fact that inter-annual variability is controlled by ablation (and thus temperature) does not necessarily imply that the decadal variability is controlled by temperature. There are known example where the (pluri-)decadal mass balance variability has been found to be related to precipitation (e.g., Vincent and others, 2005)

P1587
L13-15. Can you clarify the difference between “accelerated rates” of mass loss and “increases” losses?
L18. Barnes
L19. The number was never quoted earlier in the text.
L21-22. Probably not needed in the conclusion (?)

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P1588

L1. (Arendt and others, 2006) have published a thorough and useful analysis on different methods of extrapolating centerline elevation changes measurements. Given that the sampling by centerline altimetry itself has been challenged subsequently (Berthier and others, 2010), I am not sure this is an appropriate reference here. I do not disagree with your conclusion that discontinuous/sparse measurements of elevation changes can provide a reasonable estimate of the regional volume loss but one property is that the glacier complex needs to be randomly sampled. This is probably often the case with ICESat (as soon as the study area is large enough so that the number of tracks is sufficient). This is less obvious with a centerline sampling of a selected number of glaciers.

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Tables

Table 1. Maybe add a column with the glacier wide mass balance (a unit that is more useful than total loss to compare different glaciers/glacier complex)
Maybe it would be useful for the reader if the 5 regions Barnes, Bylot, Penny, North, South were outlined in Figure 1 (South and North is not really obvious)

Table A1. Indicate exact date of survey YYYYMMDD. Did you define C.P. somewhere?

Table A2. I would have expected close to 0 mean difference "after". Not really the case for Penny, C.P., Baffin. Reason? 1 year time difference with ICESAT? (Maybe add an explanation in the legend?)

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Figures

Figure 1. Could you zoom in? The western part of the figure does not seem to be useful.

Figure 2 (if retained). Make the vertical axis larger and the blue dots thicker

Figure 3. Add ICESat and ATM location where repeat measurements are available (in particular if you follow my General Comments #3).

Figure 6 and 7. Avoid transparency for the insets. With a white background they would be more readable (those two insets are important results probably more important than the background map). On a right axis you could show the corresponding mass balance values (also true for Fig. 10).

Figure A1.
Panes → Planes
Hard to understand the whole "bubble": "filter dH by SPOT correlation score"
Filter out outliers. Here 3 sigma. It is for each altitude interval separately? Otherwise you risk filtering out rapidly thinning or thickening glacier tongues?

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REFERENCE for my review

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